PHILOSOPHICAL TRANSACTIONS,

GIVING SOME

ACCOUNT

OF THE

Present Undertakings, Studies, *and* Labours,

OF THE

INGENIOUS,

IN MANY

Considerable Parts of the WORLD.

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LVIII. *A treatiſe on the preceſſion of the equinoxes, and in general on the motion of the nodes, and the alteration of the inclination of the orbit of a planet to the ecliptic. Inſcribed to the gentlemen of the* Royal Society, *by M.* De St. Jaques Silvabelle.

Tranſlated from the French *M. S. by* J. Bevis, *M. D.*

Introduction.

Read March 12, 1752. IF the earth were perfectly ſpherical, the action of the ſun on all the parts which compoſe it, would not produce any effect to make it turn round its centre; becauſe the moment, which would be produced on one ſide, would be always counterbalanced by an equal moment on the oppoſite ſide of the centre.

It would be the ſame, if the earth were a ſpheroid flatted at the poles, and the ſun was always in the equator, or in the ninetieth degree of declination: But in every other degree of declination its action on the exceſs of matter about the equator has a tendency to make the equator approach towards the ſun's place, or to diminiſh the angle of the ſun's declination, by making the earth's axis to turn round its centre in the plane of the circle of the ſun's declination.

The earth has then, at every inſtant, two motions of rotation; one about the axe of the equator, called alſo the earth's axe; and this is the diurnal motion,

which is uniform; the other motion of rotation is performed about the axe of the circle of the ſun's declination, which is a diameter of the equator; and this motion is produced by the action of the ſun on the redundant matter about the equator, and is continually accelerated, from the continual application of the ſolar action producing it.

The point *E*, *fig.* 1. *n°* 2. which is the interſection of the circumference of the equator and the circumference of the circle of declination, has two motions, whoſe directions are perpendicular to each other. Let *E e* be the ſpace, which it runs through in an inſtant *d t*, in the circumference of the equator, by the uniform diurnal motion, and let *E ε* be the ſpace it runs through in the ſame inſtant, in the circumference of the circle of declination, by an accelerated motion, as has been explained.

The point *E*, in vertue of theſe two motions *E e* and *E ε*, will not circulate either in the circumference *E e Q e' Q E* of the equator, or in the circumference *E ε P E' P' E* of the circle of declination, but forming the rectangular parallelogram *E e e' ε*, the diagonal *E e'* will be the elementary arc of the circumference *E e q E'*, in which the point *E* will circulate, and the angle *e E e'* will be equal to the angle *Q C q*, and equal to the angle *P' C p'* which the pole *P'* runs through in an inſtant in the circumference *P' p' Q q P Q' P'* whoſe plane is perpendicular to the plane of the circle of declination; and when the lines *E e*, *E ε* are known at every inſtant, *P' p'* will alſo be known, ſince the angle *e E e'* = to the angle *Q C q* = to the angle *P' C p'*.

The

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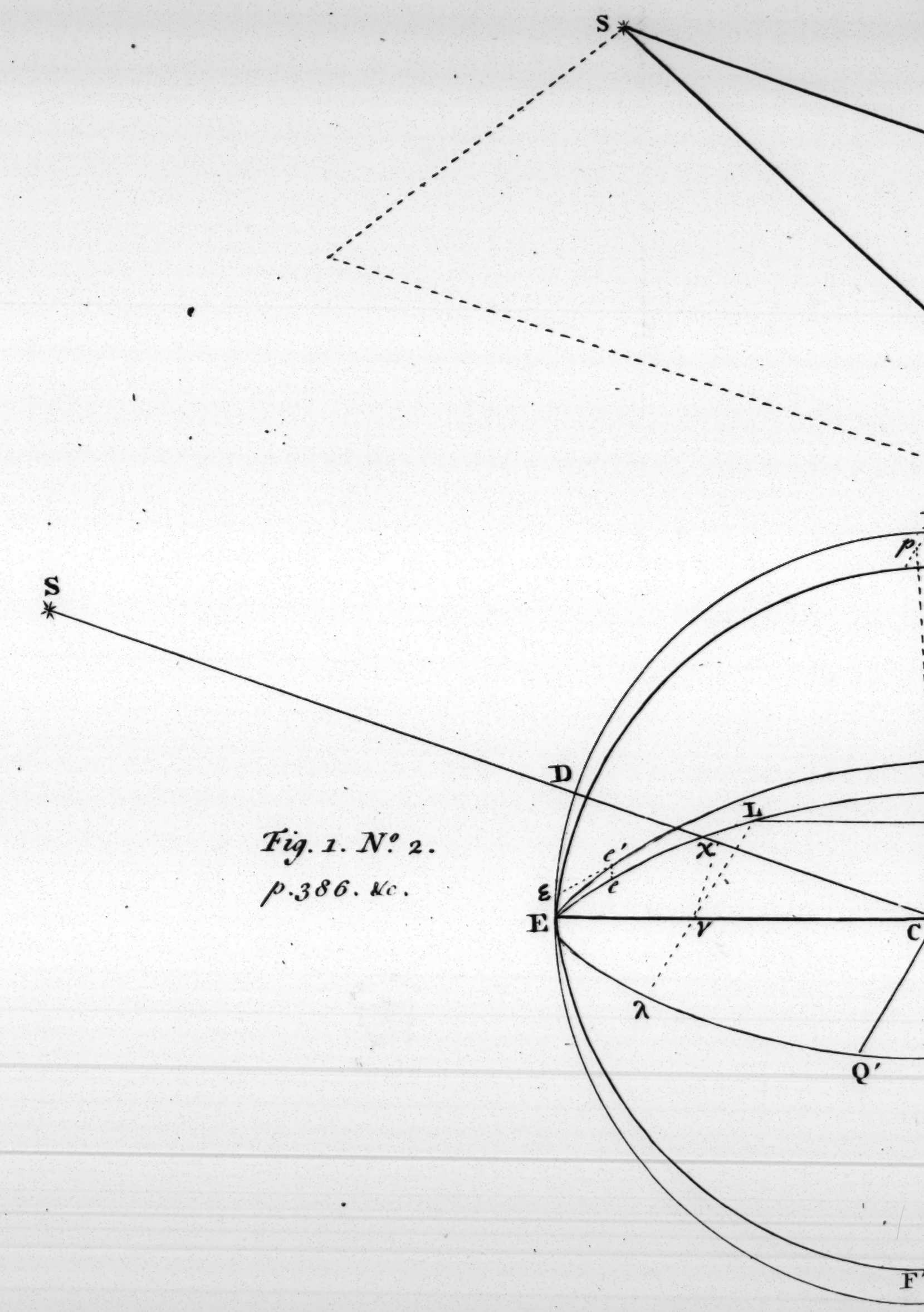
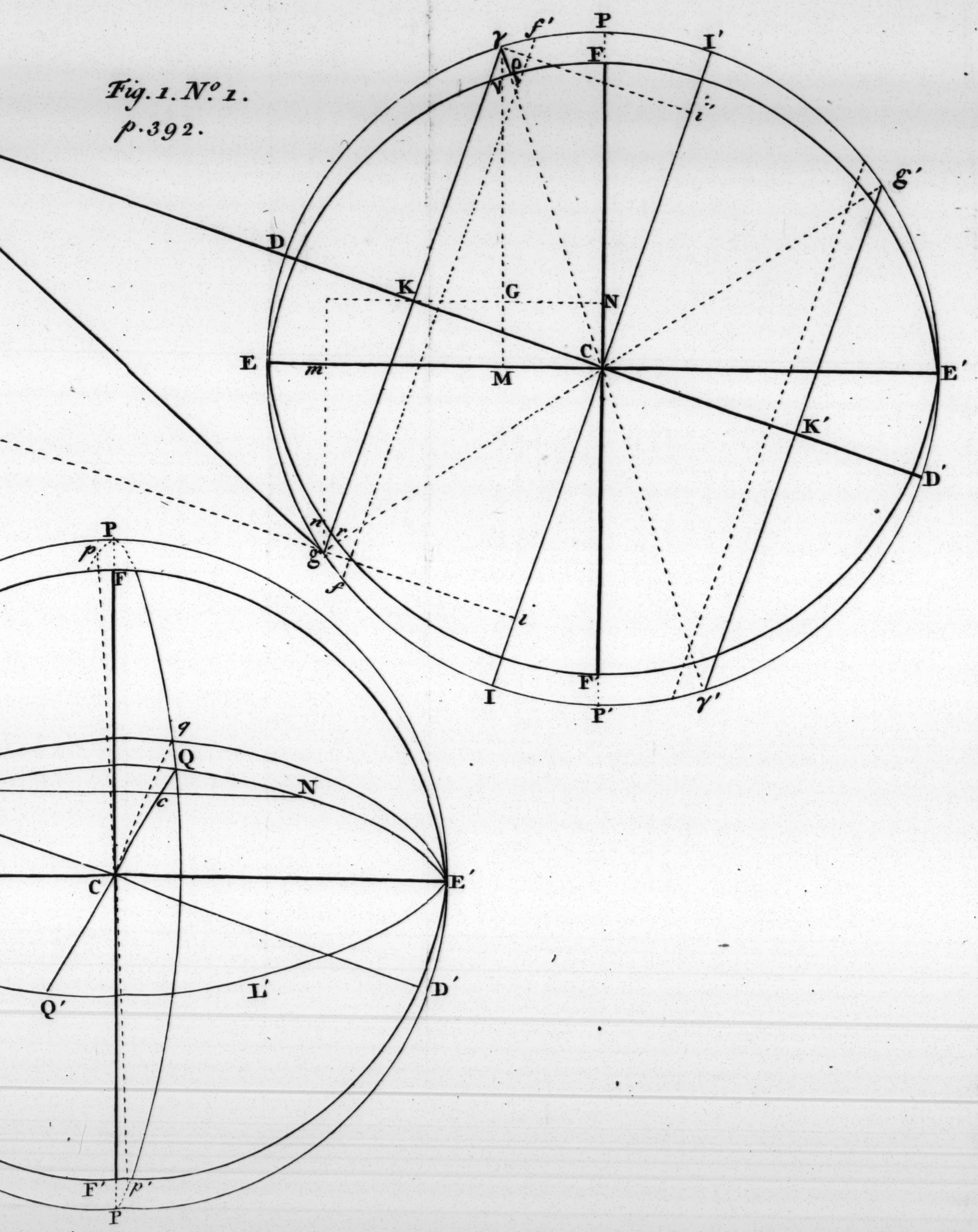


Fig. 1. N°. 2.
p. 386. &c.

Fig. 1. N.° 1.
p. 392.



J. Mynde sculp.

The inſtantaneous motion of the pole, which is $P'p'$, or Pp, *fig.* 2. *n°* 1. & *n°* 2*. may be reſolved into two, PR and PM, perpendicular to each other, and both to the earth's axe. The former cauſes the pole P to move parallel to the ecliptic ♈ ♋ ♎ ♑, and alters the place of the ſolſtice ♋, and conſequently alſo that of the equinoctial points ♈ and ♎; the latter, which is according to PM, alters the inclination of the earth's axe to the ecliptic.

To have the motion of the pole parallel to the ecliptic, or, which is the ſame, the motion of the node ♈, or the preceſſion, in the ſame time that the ſun paſſes from the equinox ♈ to the ſolſtice ♋, take the integral of the lines PR, ſuppoſing PR generally to expreſs the inſtantaneous preceſſion for any declination of the ſun S.

And to have the alteration of the inclination in the ſame time that the ſun is paſſing from ♈ to ♋, take the ſum, or the integral of the lines PM, ſuppoſing PM generally to expreſs the inſtantaneous alteration of the inclination of the earth's axe to the ecliptic for any given declination of the ſun.

The ſum of the lines PR is always the ſame, and has the ſame ſign, or the ſame direction, during every quarter of the ſun's revolution, whether he moves from ♈ to ♋, or from ♋ to ♎, or from ♎ to ♑, or from ♑ to ♈; ſo that the preceſſion anſwering to any one quarter of the ſun's revolution about the earth, or to three months, being known, that multiplied by 4 will be the annual preceſſion; by 8 will give it for two years; by 16 for 4 years, *&c.*

* *Fig.* 2. is explained at the beginning of *Prob.* 5.

Likewiſe the ſum of the lines *P M* is ever the ſame for every quarter of the ſolar revolution; but it has alternatively a contrary ſign; that is, a contrary direction. During the quarter from ♈ to ♋, the alteration of the inclination of the earth's axe to the ecliptic is poſitive, and the angle of the inclination increaſes; but during the ſucceeding quarter, or from ♋ to ♎, the alteration of the inclination is negative, and the angle of the inclination diminiſhes: And as the diminution from ♋ to ♎ is equal to the augmentation from ♈ to ♋, it follows, that at the end of the ſemirevolution the inclination of the earth's axe to the plane of the ecliptic will become again the ſame, having undergone an oſcillation, which is completed in a ſemirevolution. It is the ſame, when the ſun paſſes from ♎ to ♈. The angle of the inclination increaſes from ♎ to ♑, and decreaſes from ♑ to ♈, where it becomes again the ſame it was at ♎.

And hence the inclination of the earth's axe to the ecliptic may be conſidered as conſtant, tho' ſubject to this oſcillation, and indeed to ſeveral others, which will be preſently explained, they being all regular, and performed in regular periods.

The earth's inclination to the ecliptic being conſtant, and the motion of the pole which produces the preceſſion, being always parallel to the plane of the ecliptic, the earth's pole moves in a parallel to the ecliptic, about 23 degrees and a half diſtant from the pole of the ecliptic, and the terreſtrial axe deſcribes a conic ſurface.

To this motion of the terreſtrial axe or pole is to be aſcribed the apparent motion of the ſtars about the pole of the ecliptic.

But

But hitherto we have not conſidered, that to the preceſſion, thus cauſed by the ſun, we are to add that likewiſe produced by the moon; and it remains, that we examine into the motion of the earth's pole, cauſed by the action of the moon on the redundant matter about the earth's equator.

All, that has been ſaid concerning the ſun, is alike applicable to the moon, which we may put in the place of the ſun; the moon's orbit in the place of the ecliptic; and the time of the moon's revolution round the earth in the place of the revolution of the ſun round the earth: And we ſhall find the motion of the earth's pole parallel to the lunar orbit, which is always the ſame at every quarter of the time of the revolution of the moon round the earth, and the oſcillation of the earth's axe to the plane of the lunar orbit, which is completed in each ſemirevolution of the moon round the earth.

But whereas the plane of the lunar orbit, which is always inclined to the plane of the ecliptic in an angle of about 5 degrees, never continues in a conſtant poſition, like the plane of the ecliptic, ſo that its pole deſcribes a ſmall circle parallel to the ecliptic, at the diſtance of about 5 degrees from the pole thereof; it follows, that the preceſſion, with reſpect to the lunar orbit, is not the ſame as with reſpect to the ecliptic; and that the motion of the pole parallel to the lunar orbit ſhould be referred to the plane of the ecliptic: Which is done by reſolving the motion of the pole, parallel to the plane of the lunar orbit, into two motions, the one parallel to the plane of the ecliptic, and the other perpendicular thereto, and in the plane of the ſolſticial colure.

The

The former of theſe two motions gives the preceſſion with reſpect to the ecliptic, and has its direction always the ſame way.

The latter motion has two oppoſite directions, in the two ſemirevolutions of the pole of the lunar orbit round the pole of the ecliptic, and cauſes an oſcillation of the terreſtrial axe on the plane of the ecliptic, which is compleated in a revolution of the pole of the lunar orbit round the pole of the ecliptic.

From all that has been ſaid, it follows, that there are five diſtinct motions of the pole of the earth; namely, two of preceſſion, which are parallel to the plane of the ecliptic, and three of oſcillation on the plane of the ecliptic.

The two of preceſſion are cauſed, the one by the ſun, the other by the moon. That, which is cauſed by the ſun, is conſtantly the ſame at every quarter of the time of the revolution of the ſun round the earth, that is, every three months: That which is cauſed by the moon, is conſtantly the ſame at every quarter of the time of the revolution of the moon round the earth; that is, about every ſeven days.

Of the three motions of oſcillation, one is cauſed by the ſun, and is completed in the time of the ſemirevolution of the ſun round the earth, taken from one equinox to the following one; that is, in ſix months.

The other is cauſed by the moon; and each oſcillation is completed in the ſpace of a ſemirevolution of the moon round the earth; that is, in about 14 days.

The third is cauſed likewiſe by the moon, and ariſes from the plane of her orbit being different from the plane of the ecliptic, and from the pole of the lunar orbit making its revolution about the pole of the ecliptic

ecliptic in about 18 years and two thirds. And this oſcillation is compleated in the time of the revolution of the pole of the lunar orbit about the pole of the ecliptic; that is, in about 18 years and two thirds.

It will appear in the memoir, that there is a relation purely geometrical between the quantity of the nutation, during the time of the ſemirevolution of the pole of the lunar orbit, and the quantity of the preceſſion, cauſed likewiſe by the moon in the ſame time. This relation is quite independent of the force of the moon, of the quantity of the earth's flatneſs, of the quantity of the terreſtrial matter, and, in a word, of every thing of a phyſical nature that can enter into the problem.

We are content to examine the motions of the pole of the earth produced by the ſun and the moon. The ſame method, and the ſame *formulæ*, will give likewiſe the motions of the terreſtrial pole ariſing from any other planet, as Saturn, Jupiter, &c. but theſe motions are too minute to merit attention.

Whatever has been ſaid of the action of the ſun on the redundant matter about the earth's equator, is alſo applicable to his action on a ſimple ring placed at the equator, without adhering to the terreſtrial globe; and the motion of the pole of ſuch ring may be determined by the ſame method, and conſequently the motion of its nodes on the plane of the ecliptic, and the alteration of the inclination of its axe to the ſame plane. And ſince theſe motions are the ſame, whether the ring be ſuppoſed entire, or a ſmall portion of it only be conſidered, or a mere point thereof, the motions of the nodes, and the alteration of the inclination of a moon, or a ſatellite of a planet, may thereby

thereby be known. And the *formulæ* differ in nothing from thoſe of the motion of the nodes of the earth's equator, and of the alteration of the obliquity of the earth's axe to the plane of the ecliptic, but in this; that the action of the ſun on the ring to make it turn, is exerted entirely thereon; whereas in the problem of the preceſſion this force muſt neceſſarily be diſtributed throughout the whole maſs of the earth, on account of the adherence of the ring to the globe of the earth.

DIVISION of the WORK.

This memoir is divided into four ſections.

The 1ſt ſection treats of the motion of the pole of the terreſtrial equator cauſed by the ſun.

The 2d ſection treats of the motion of the pole of the terreſtrial equator cauſed by the moon.

The 3d ſection treats of the motion of the pole of a ring, or of the orbit of a moon, cauſed by the ſun.

The 4th ſection contains the application of the *formulæ* found in the other ſections.

SECT. I.

Of the motion of the pole of the terreſtrial equator cauſed by the action of the ſun.

PROBLEM I.

Article 1. *To find the moment, which reſults from the attraction of each particle of the earth towards the ſun, in the inverſe ratio of the ſquare of the diſtance, to make the earth's axis turn upon its centre* C.

Let *E F E' F E*, *fig.* 1. *n°* 1. be the ſection of the earth by the plane of the circle of the ſun's declination;

tion; $P E P' E' P$ the circumference whoſe diameter is the earth's equator, $E E'$ or $P P'$. Let $E E'$ be perpendicular to $P P'$, and in the equator. Let $I I'$ be perpendicular to $C S$, and aſſume this diameter $I I'$ for the leaver to which all the moments are to be referred.

The motion of any point, as g, towards the ſun, is $\frac{S}{\overline{Sg}^2}$. By reſolving this motion into two, one according to gC, the other parallel to CS; the motion according to gC has no tendency to make the point g turn round the centre C. The motion of the point g parallel to CS will be $\frac{S}{\overline{Sg}^2} \times \frac{SC}{Sg}$ or $\frac{S \times SC}{\overline{Sg}^3}$; but the motion of the centre C according to CS is $\frac{S}{\overline{SC}^2}$; therefore the relative motion of the point g, in regard gard to the centre C, is $\frac{S \times SC}{\overline{Sg}^3} - \frac{S}{SC}$ or $S \times SC \times \left(\frac{1}{\overline{Sg}^3} - \frac{1}{\overline{SC}^3}\right)$.

The moment of the point g to turn about the centre C, will therefore be $S \times SC \times \left(\frac{1}{\overline{Sg}^3} - \frac{1}{\overline{SC}^3}\right) \times g \times CI$, the moment being the product of the motion by the maſs of the body, and by the arm of the leaver. And this moment cauſes the point I to approach towards the ſun, when it is poſitive, or when SC is greater than Sg; and cauſes the point I to recede from the ſun when it is negative, or when Sg is greater than SC.

It will be found, in like manner, that the moment of the point g' to turn about the centre C, is $S \times SC \times \left(\frac{1}{\overline{Sg'}^3} - \frac{1}{\overline{SC}^3}\right) \times g' \times CI'$; or with regard to the point I which is on the oppoſite ſide of the centre to the point I', this moment, by changing the ſigns, will be, $S \times SC \times \left(\frac{1}{\overline{SC}^3} - \frac{1}{\overline{Sg'}^3}\right) \times g' \times CI' = S \times SC \times \left(\frac{1}{\overline{SC}^3} - \frac{1}{\overline{Sg'}^3}\right) \times g \times CI$. Therefore the moment of the two equal points g and g' to cauſe the point I to turn about the centre C, is $S \times SC \times \left(\frac{1}{\overline{Sg}^3} - \frac{1}{\overline{SC}^3}\right) \times g \times CI + S \times SC \times \left(\frac{1}{\overline{SC}^3} - \frac{1}{\overline{Sg'}^3}\right) \times g \times CI = g \times CI \times S \times SC \times \left(\frac{1}{\overline{Sg}^3} - \frac{1}{\overline{Sg'}^3}\right)$.

If the points γ and γ' be taken equally diſtant from the points D and D', as the points g and g', it will likewiſe be found, that the moment of the equal points γ and γ' to cauſe the point I to turn about the centre C, is $\gamma \times \overline{CI} \times S \times \overline{SC} \times \left(\frac{1}{\overline{S\gamma'}^3} - \frac{1}{\overline{S\gamma}^3}\right) = \gamma \times \overline{CI} \times S \times SC \times \left(\frac{1}{\overline{Sg}^3} - \frac{1}{\overline{Sg'}^3}\right)$.

Therefore the moment of all the four points together, g, g', γ, γ', to cauſe the point I to turn about the centre C, is $(g - \gamma) \times \overline{CI} \times S \times SC \times \left(\frac{1}{\overline{Sg}^3} - \frac{1}{\overline{Sg'}^3}\right)$, which

which becomes = o when the point g can be taken = γ, as may be done throughout the whole extent of the circle $PEP'E'P$; but if the zone $EP'E'PEF'E'FE$ be taken away from this circle, to have the true figure of the earth, we ſhall have $g = \overline{gf} \times \overline{gr}$, and $\gamma = -\overline{\gamma f} \times \overline{\gamma\varrho}$, taking the points g and γ for the elements of the zone, and $g - \gamma = \overline{gf} \times \overline{\gamma\varrho} - \overline{gr}$, becauſe $\overline{\gamma f} = \overline{gf}$.

Let the lines CS and CP, or CE, be called s and a, reſpectively; the radius 1, or unity; the ſine of the angle SCP, V; its coſine, or the fine of the angle SCE, u; CK, x; gK, y; $\overline{gm}$, z; $\overline{\gamma M}$, z'; $\overline{gf}$ or $\overline{\gamma f}$, du; PF, $a\,a$; we ſhall have $gn = a\,z$; $\gamma v = a\,z'$ (by the property of the ellipſe); alſo calling $\overline{gr}$, r; and $\gamma\varrho$, ϱ; and regarding the little triangle grn as a right-lined one, and ſimilar to the triangle gCm, we ſhall have $r = \frac{z}{a} \times a\,z$, and likewiſe $\varrho = \frac{z'}{a} \times a\,z'$.

By the proportion of fines to the ſides of triangles, we ſhall have $\gamma G = y\,V$: $CN = x\,u$. Finally, if we conſider the point S, or the ſun, as at an infinite diſtance, we ſhall have $Sg = SK = SC - CK = s - x$, and $Sg' = SK' = SC + CK' = s + x$.

Therefore $\frac{1}{\overline{Sg}^3} = \frac{1}{s^3 - 3s^2x + 3sxx - x^3}$, and $\frac{1}{\overline{Sg'}^3}$ $= \frac{1}{s^3 + 3s^2x + 3sx^2 + x^3}$. Therefore $\frac{1}{\overline{Sg}^3} - \frac{1}{\overline{Sg'}^3} =$ $\frac{6s^2x}{}$

$\frac{6s^2x + 2x^3}{(s-x)^3 \times (s+x)^3}$; and, rejecting the infinitely ſmall magnitudes, $= \frac{6x}{s^4}$.

Therefore the moment wherewith the four points g, g', γ, γ', cauſe the point I to turn, will, by ſubſtituting in the place of the letters their analytical values, be $(\varrho\, du - r\, du) \times S \times y \times \frac{6x}{s^3}$; or putting, inſtead of ϱ and r, the values above found, we ſhall have for the moment $\left(\frac{a}{a} z'z' - \frac{a}{a} zz\right) \times du \times S \times y \times \frac{6x}{s^3} = \frac{6\,a\,S}{s^3 a} \times y\, x\, du \times (z' + z) \times (z' - z)$. But $z' + z = \gamma M + g m = 2\,\overline{\gamma G} = 2\,y\,V$; and $z' - z = \gamma M - g m = 2\,\overline{GM} = 2\,\overline{CN} = 2\,x\,u$. Therefore $(z' + z) \times (z' - z) = 4\,V\,u\,x\,y$; and the moment is $\frac{6aS}{s^3} \times 4\,V\,u \times x\,x\,y^2\,du = \frac{6aS}{s^3} \times 4\,V\,u \times x\,x\,dx\,u$. But in the circle $D\,g\,I$ we have $y\,du = a\,dx$, and the moment is $\frac{6aS}{s^3} \times 4\,V\,u \times x\,x\,y\,dx = \frac{6aS}{s^3} \times 4\,V\,u \times dx \sqrt{aa - xx}$. But the integral of $x^2\,dx\,\sqrt{aaxx}$ is $\frac{1}{4}\, a\,a \int dx \sqrt{aa - xx} - \frac{x}{4}(a\,a - x\,x)^{\frac{3}{2}}$, and when $x = a$, this integral is $\frac{1}{4}\, a\,a \int dx \sqrt{aa - xx}$, or, calling the ratio of the circumference to the diameter π, we ſhall have $\int dx \sqrt{aa - xx}$, or the area of the quadrant whereof a is the radius, $= \frac{1}{2}\, a\,\pi \times \frac{1}{2}\, a = \frac{1}{4}\, a^2\,\pi$. Therefore the integral of $\frac{6a'S}{s^3} \times 4\,V\,u \times x\,x\,dx \sqrt{aa}$

$\sqrt{aa-xx}$, or the ſum of the moments of all the points which compoſe the elliptic zone $E\,P'\,E'\,P\,E\,F\,E'\,F\,E$ is $\frac{6\,a\,S}{s^3} \times \frac{V\,u\,\pi\,a^4}{4}$.

If the terreſtrial ſpheroid be cut by any plane parallel to the plane $P\,E\,P'\,E'\,P$ of the circle of declination, *fig.* 1. *n°* 2. the ſection will be an ellipſe ſimilar to the ellipſe $E\,F\,E'\,F\,E$; and if the greater ſemiaxe $c'\,L$ of this ellipſe be called X, it will be found, as has been already in the ellipſe $E\,F\,F\,F\,E$, that the ſum of the moments of all the points of this ellipſe to turn about its centre C, is $\frac{6\,a\,S}{S^3} \times \frac{V\,u\,\pi\,X^4}{4}$.

It has not been taken into conſideration, that here the centre S of the ſun is a little below the plane of the ſection, the line $c'\,S$ making an inſenſible angle with the line $C\,S$.

Calling $C\,c'$, Υ; the moment of all the ſections parallel to $E\,F\,E'\,F\,E$ to turn about the axe $C\,c'$ will be the integral of $\frac{3\,a\,S}{S^3} \times \frac{V\,u\,\pi\,X^4}{2} \times d\,\Upsilon$, or the integral of $\frac{3\,a\,S}{s^3} \times \frac{V\,u\,\pi}{2} \times (aa\,\Upsilon\Upsilon)^2 \times d\,\Upsilon = \frac{3\,a\,S}{s^3} \times \frac{V\,u\,\pi}{2} \times (a^4\,d\,\Upsilon - 2a^2\,\Upsilon^2\,d\,\Upsilon + \Upsilon^4\,d\,\Upsilon)$, and this integral is $\frac{3\,a\,S}{s^3} \times \frac{V\,u\,\pi}{2} \times (a^4\,\Upsilon - \frac{2}{3}\,a^2\,\Upsilon^3 + \frac{1}{5}\,\Upsilon^5)$; and when $\Upsilon = a$, the integral is $\frac{3\,a\,S}{s^3} \times \frac{V\,u\,\pi}{2} \times \frac{8}{15}\,a^5$. This is the ſum of the moments of all the points of the hemiſpheroid of the earth formed by the ſection $E\,P'\,E'\,P\,E$ of the circle of declination, and the ſum of the moments of all the points of the whole ſpheroid

roid to turn about the axe $\mathcal{Q}\mathcal{Q}'$ is $\frac{3aS}{s^3} \times Vu\pi \times \frac{8}{15}a^5$. Which was to be found.

Corollary.

2. If it were required to find the ſum of the moments of all the points of a ſingle crown $ELQE'Q'E$, *fig.* 1. *n°* 2. placed at the equator, and detached from the earth, to turn about the axe $\mathcal{Q}\mathcal{Q}'$ of the circle of the ſun's declination, the thickneſs of the crown being a a:

The moment of the point L to turn about c' is the ſame as that of the point γ to turn about C; as is manifeſt from the reſolution of the motion according to SL into two, $L\gamma$, and γS*; and from the great diſtance of the ſun γS will not differ from LS; the triangle $L\gamma S$ being right-angled at γ.

By the preceding problem, if $c'L$, or $C\Upsilon$, be called X; and Cc', Υ; and all the other denominations of that problem be retained; we ſhall have $\gamma\varkappa = Xu$, and $C\varkappa = XV$.

And the moment of the point γ is $\gamma \times \gamma\varkappa \times S \times \overline{SC} \times \left(\frac{1}{\overline{S\gamma}^3} - \frac{1}{\overline{SC}^3}\right) = \gamma \times Xu \times S \times \frac{3XV}{s^3}$.

And putting inſtead of γ, the element of the crown, which is $a\,a \times du$, calling, in this caſe, du the element of the circumference $ELQE'$, the moment of an element of the crown will be $aa \times \frac{3S}{s^3} \times Vu \times$

* The lines SL and γS are not drawn in the *fig.* to avoid confuſion.

$XXdu$. But in the circle $EQE'E$ we have, $Xdu = a\,d\Upsilon$; therefore the moment of an element of the crown is $aa \times \frac{3S}{s^3} \times Vu \times a \times d\Upsilon = \frac{3S}{s^3} \times aa^2\,Vu \times d\Upsilon\sqrt{aa - \Upsilon\Upsilon}$; and the ſum of the moments of the quadrant ELQ of the crown is $\frac{3S}{s^3} \times aa^2\,V\,u \times \frac{1}{2}a\,\pi \times \frac{1}{2}a = \frac{3S}{s^3} \times a\,Vu \times \frac{1}{4}\pi\,a^4$. And every other quadrant of the crown having an equal motion, the ſum of the moments of all the points of the crown, to turn, in vertue of the ſolar action, about the axe QQ', is $\frac{3S}{s3} \times a\,Vu \times \pi\,a^4$.

Problem II.

3. *To find the ſum of the moments of all the points of the terreſtrial ſpheroid turning about the axe* QQ', *the motion of the point* E *being given, and* $= \mu$.

We muſt firſt ſeek the ſum of the moments of all the points of the ellipſe $EF'E'FE$, *fig.* 1. n° 2. turning about its centre C; and to have the ſum of the moments of all the points of this ellipſe, we ſhall firſt find the ſum of the moments of all the points of the circle $EP'E'PE$, and then we ſhall ſubduct therefrom the ſum of the moments of the whole elliptic zone $EP'E'PEF'E'FE$.

The motion of any point of the circle $EP'E'PEC$, placed at the diſtance x from the centre C, will be $\frac{x}{a}\mu$; the motions being here in the ſame ratio as the di-

ſtances

ſtances from the centre, ſince all the points complete their revolution in the ſame time; and the moment of all the points which compoſe the circumference $2\pi x$ will be $2\pi x \times \frac{x}{a}\mu \times x$; the moment being produced from the maſs by the motion, and by the arm of the leaver.

Therefore the motion of all the concentric circumferences which compoſe the circle, is $\int \frac{2\pi\mu}{a} \times x^3 dx$

$= \frac{1}{2}\frac{\pi\mu}{a} \times x^4 = \frac{1}{2}\frac{\pi\mu}{a} \times a^4$, when $x = a$, as for the circle $E P' E' P E$.

To have the ſum of the moments of all the points of the elliptical zone $E P' E' P E F' E' F E$, all the points of the zone may be conſidered as placed at the diſtance a, from the centre C, and having, conſequently, the ſame motion as the point E, becauſe the greateſt thickneſs $P F$ of the zone is very ſmall.

The ſum of the moments of the zone will be equal to the quantity of matter multiplied by the motion μ, and by the arm of the leaver a, or $2a\pi \times \frac{1}{2}a u \times \mu \times a = \pi\mu a a^3$.

Therefore the ſum of the moments of all the points of the ellipſe $E F' E' F E$ turning about its centre C, the motion of the point E being μ, will be

$\frac{1}{2}\frac{\pi\mu}{a} \times a^4 - \pi\mu a a^3$.

It will likewiſe be found generally, that the ſum of the moments of a ſimilar ellipſe, of which $C' L$, or X, is half the greater axe, and which turns about its centre C', is $\frac{1}{2}\frac{\pi\mu}{a} \times X^4 - \frac{\pi\mu a}{a} \times X^4$.

Therefore

Therefore the ſum of all the ſimilar ellipſes which compoſe the terreſtrial hemiſpheroid, is $\int \frac{1-2a}{2a} \times \pi \mu \times X^4 \, d\Upsilon = \frac{1-2a}{2a} \times \pi \mu \times \int (aa - \Upsilon\Upsilon)^2 \times d\Upsilon$, which, as has been ſeen at the end of *Prob.* I. when, after the integration Υ has been made $= a$, will be $\frac{1-2a}{2a} \pi \mu \times \frac{8}{15} a^5$, and multiplying by 2, the ſum of all the points of the terreſtrial ſpheroid will be $(1-2a) \times \pi \mu \times \frac{8}{15} a^4$. Which was to be found.

COROLLARY.

4. If you would find the ſum of the moments of all the points of the ring $E \mathcal{Q} E' \mathcal{Q}' B$, *fig.* 1. *n°* 2. turning about the axe $\mathcal{Q}\mathcal{Q}'$:

The moment of an element L of the crown placed at the diſtance X from the axe of rotation $\mathcal{Q}\mathcal{Q}'$, is $a \, a \, du \times \frac{\mu X}{a} \times X$, which is the product of the quantity of matter by the motion, and by the arm of the leaver. But in the circle $E \mathcal{Q} E' E$ we have, $du \frac{a \, d\Upsilon}{X}$; therefore the moment of an element of the crown is $a \, a \, \mu \times X \, d\Upsilon$, whoſe integral, which is $a \, a \, \mu \times a \, a \, \pi$, gives the ſum of the moments of all the points of the crown. Which was to be found.

Problem III.

5. *Having given, the moment wherewith the earth turns about the axe* QQ' *of the circle of the ſun's declination, To find the motion of the pole* P, *or, which is the ſame, of the point* E *in the plane of the circle of the ſun's declination.*

By *Prob.* I. the moment wherewith the earth turns about the axe CQ, by the action of the ſun, is $\frac{3S}{s^3} \times a\,\pi\,\nu\,u \times \frac{8}{15}a^5$.

By *Prob.* II. the moment wherewith the earth turns about the ſame axe CQ, the velocity of the point P, or of the point E, being μ, is $(1-2a) \times \pi\,\mu \times \frac{8}{15}a^4$.

We ſhall have then $\frac{3S}{s^3} a\,\pi\,\nu\,u \times \frac{8}{15}a^5 = (1-2a) \times \pi\,\mu \times \frac{8}{15}a^4$; whence we get $\mu = \frac{3\,S\,a\,a\,\nu u}{s^3 \times (1-2a)}$. And this is the motion of the pole P, or of the point E, about the centre C. Which was to be found.

Corollary.

6. In like manner, the moment wherewith the crown $EQE'Q'E$ turns about the axe QQ' being given, the motion of the point E may be known. For,

By the *Coroll.* of *Prob.* I. the moment of this crown, turning about the axe CQ by the action of the ſun, is $\frac{3S}{s^3} \times a\,\pi\,\nu\,u \times a^4$.

By

By the *Coroll.* of *Prob.* II. the moment of this crown is alſo a $a\mu \times a^2\pi$.

We have then $\frac{3S}{s^3} \times a\pi\nu u \times a^4 = a a \mu \times a^2 \pi$;

whence we get $\mu = \frac{3S}{s^3} \times a\nu u$. Which was to be found.

Remark. It is proper to obſerve, that the motion of the point *E* of the crown is the ſame, whether the crown be entire, or there be no more of it but the point *E*.

For, by *Prob.* I. the motion of any point *g*, parallel to *C S*, *fig.* 1. *n°* 2. and reſpectively to the centre *C*, is $S \times \overline{SC} \times \left(\frac{1}{\overline{Sg}^3} - \frac{1}{\overline{SC}^3}\right)$, which is $= S \times \frac{3x}{S^3}$; and when the point *g* becomes the point *E*, we have $x = a\nu$, ν being the coſine of the angle *D C E*. Therefore the reſpective motion of the point *E* parallel to the motion *C S* of the centre *C*, is $S \times \frac{3a\nu}{S^3}$; and reſolving this motion into two, the one according to *E C*, and the other perpendicular to *E C*, this latter will be $= S \times \frac{3a\nu}{s^3} \times \frac{au}{a} = \frac{3S}{s^3} a\nu u$, being the ſame as that which was found for the point *E* when it is united to the crown.

This ſhews that the motion of the other points of the crown produce no alteration in the motion of the point *E*, which is owing to this; that all the points of the crown have the ſame angular motion about the axe *C Q*.

Problem IV.

7. To find at every inſtant the variation of the place of the pole of the earth.

The earth has two motions of rotation, the one about the axe of the equator, which is likewiſe the earth's axis; and the other about the axe $C Q$ of the circle of the ſun's declination; this motion is cauſed by the ſun's action on the redundant matter about the equator.

The point E, *fig.* 1. *n°* 2. which is the interſection of the circumference $E Q E' Q' E$ of the equator and the circle $P E P' E' P$ of the ſun's declination, will therefore have at every inſtant two motions, whoſe directions are perpendicular to each other.

The former of theſe two motions, which is the diurnal motion, is uniform; and if we call it m, the ſpace run through in an inſtant, dt, is $m\,dt$; the motion being always equal to the ſpace divided by the time, or the ſpace equal to the product of the time. Alſo let $E e$ be that ſpace.

The ſecond motion of the point E which is performed in the circumference $E P E' P' E$ of the circle of the ſun's declination, and ariſes from the action of the ſun, is continually accelerated, from the continual application of the ſun's action; and if we call the initial motion μ, at the firſt term of the inſtant, dt, or the increment of the motion, the motion at the end of the inſtant dt, is $\mu\,dt$; and the ſpace gone thro' uniformly by the motion during the inſtant dt, is $\mu\,dt \times dt$, or $\mu\,dt^2$. Let $E \varepsilon$ be that ſpace.

The

The point E, in conſequence of theſe two motions together, $E\,e$ and $E\,\varepsilon$, circulates neither in the circumference $E\,\mathcal{Q}\,E'\,\mathcal{Q}'\,E$ of the equator, nor in the circumference $E\,P\,E'\,P'\,E$ of the ſun's declination. But if we form the rectangle $E\,e\,e'\,\varepsilon\,E$, the diagonal $E\,e'$ will be the elementary arc of the circumference $E\,e'\,q\,E'\,E$ wherein the point E will circulate; and the angle $e\,E\,e'$, equal to the angle $\mathcal{Q}\,c\,q$, equal to the angle $P'\,C\,p'$, will be the angle whereby the pole P' is elevated, and the pole P depreſſed, below the circle of declination, by moving in the circumference $P'\,p'\,\mathcal{Q}\,q\,P\,p\,\mathcal{P}'$, whoſe plane is perpendicular to the plane of delination; and the point p' is the true place of the pole at the end of the inſtant $d\,t$, and the ſmall arc $P'\,p'$ expreſſes the inſtantaneous variation of the place of the earth's pole. Which was to be found.

Corollary I.

8. The ſimilar ſectors $e\,E\,e'$, $\mathcal{Q}\,C\,q$, or $P'\,C\,p'$, whoſe three ſides of the one are each parallel to the three ſides of the other, give this proportion, $E\,e$: $e\,e'$ or $E\,\varepsilon$ :: $C\,P'$: $P'\,p' = \frac{E\,\varepsilon}{E\,e} \times C\,P' =$ (by the preceding *Prob.*) to $\frac{\mu\,d\,t^2}{m\,d\,t} \times C\,P' = \frac{\mu\,d\,t}{m} \times a$. But by *Problem* III. $\mu = \frac{3\,S\,\mathrm{a}\,a\,\nu\,u}{s^3\,(1-2\mathrm{a})}$; therefore $P'\,p' = \frac{3\,S\,\mathrm{a}\,a\,\nu\,u}{s^3 \times m \times (1-2\mathrm{a})} \times a\,d\,t = A\,\nu\,u\,a\,d\,t$, making $A = \frac{3\,S\,\mathrm{a}\,a}{s^3 \times m \times (1-2\mathrm{a})}$.

Corol-

Corollary II.

9. If, inſtead of the terreſtrial ſpheroid, we ſuppoſed only a ſingle ring, it would be found in like manner, that the pole P of ſuch ring would ſhift its place during the inſtant dt, by running through the arc $P'p' = \frac{E\varepsilon}{Ee} \times CP' = \frac{\mu\, dt}{m} \times a$. But then we ſhould take the value of μ as in *Coroll.* of *Prob.* III. which is $\frac{3S}{s^3} \times a_{\gamma} u$. Therefore we ſhall have $P'p' = \frac{\mu\, dt}{m} \times a = \frac{3\,S\,a}{s^3 \times m} \times {}_{\gamma}u\,a\,dt$.

To apply, then, the *formula* of the preced. *Coroll.* it will ſuffice to make $\frac{a}{1-2a}$ in that *Coroll.* $= 1$. for then the *formula* of *Coroll.* I. becomes the ſame with that juſt now found in this *Coroll.*

This will be of uſe hereafter for applying the *formulæ* of the motion of the nodes of the terreſtrial equator to the motion of the nodes of a moon or ſatellite of a planet.

Corollary III.

10. It ſhould be well obſerved in the preceding *Coroll.* that the direction $P'p'$ of the pole P' is ever parallel to the direction Ee of the point E of the equator, ſince they are both perpendicular to the plane of the circle of the ſun's declination, and the direction $P'p'$ goes on the ſame ſide as the direction Ee of the point E, which is placed on the ſame ſide of the

the pole P', with reſpect to the line $S\,C\,D'$, and the direction of the oppoſite pole P, goes on the ſame ſide as the direction of the point E of the equator which is placed on the ſame ſide of the pole P with reſpect to the line $S\,C\,D'$. This ſhould be carefully attended to in regard to the following *Problem*.

PROBLEM V.

11. *To determine at every inſtant the motion of the pole of the earth with reſpect to the ecliptic.*

$P\,P'$, *fig.* 2. n° 1. & n° 2*. is the earth's axe; ♈ ♋ ♎ ♑ repreſents the ecliptic; ♋ $E'\,p\,E$ ♋ is the tangent plane to the pole P, and parallel to the equator; the line D ♋ D^{2} is the interſection of this plane with that of the ecliptic, and is tangential at the common point ♋ to the circles ♋ $E'\,p\,E$ ♋ and ♈ ♋ ♎ ♑. The angle $P\,C$ ♋ is the angle of inclination of the earth's axe to the plane of the ecliptic, being likewiſe the ſolſticial angle. The earth, by its diurnal motion, turns about the axe $P'\,P$ according to the order of ♋ $E'\,p\,E$ ♋; and the ſun, by the annual motion in the ecliptic, moves according to the order of ♈ ♋ ♎ ♑.

Let S be any place of the ſun in the ecliptic. By *Art.* 10. the motion of the pole P making the acute angle $P\,C\,S$, will be according to $P\,p$ perpendicular to the plane $P\,C\,S\,D\,P$, and conſequently to the line $P\,D$.

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* *Note*, That the *fig.* n° 2. is only to repreſent the circle ♋ $E\,p$ ♋, which could not be repreſented but by projection in *fig.* n° 1.

Forming, therefore, the rectangle $P\,M\,p\,R$, the motion of the pole according to $P\,p$ will be reſolved into two, according to $P\,R$ and $P\,M$.

The motion of the pole according to $P\,R$ is parallel to the plane of the ecliptic, and to the line of the nodes ♈ C ♎. This motion tends to move the pole P about the axe of the ecliptic, and in a parallel to the ecliptic, and cauſes the equinoctial point ♈ to recede, and the point ♑, and the node ♎, to advance towards the ſun; which produces a preceſſion of the equinox equal to the angular motion of the pole P about the axe of the ecliptic; for it is indifferent whether the pole P of the earth moves about the axe of the ecliptic, whilſt the plane ♈ ♋ ♎ ♑ remains fix'd; or the ſame be ſuppoſed to be fix'd, and the plane of the ecliptic ♈ ♋ ♎ ♑ to turn about its centre C, and the axe of the ecliptic.

If we take another place S^2 of the ſun at the ſame diſtance from the ſolſtice ♋, but on the ſide contrary to S, the direction of the pole P will be, by *Art.* 10. according to $P\,p^2$, perpendicular to the plane $P\,C\,S^2\,D^2\,P$, and conſequently to the line $P\,D^2$; and the ſun's declination being the ſame in D^2 as in D, the motion $P\,p^2$ will be equal to the motion $P\,p$.

Reſolving the motion according to $P\,p^2$ into two, according to $P\,K$ and $P\,M'$, the motion according to $P\,R$ produces the ſame preceſſion, and with the ſame ſign, or the ſame direction, as when the ſun was on the other ſide of the ſolſtice ♋, and in D; and the preceſſion is ever the ſame at every quarter revolution of the ſun.

The motion according to $P\,M'$ cauſes the ſame variation in the inclination of the earth's axe to the ecliptic,

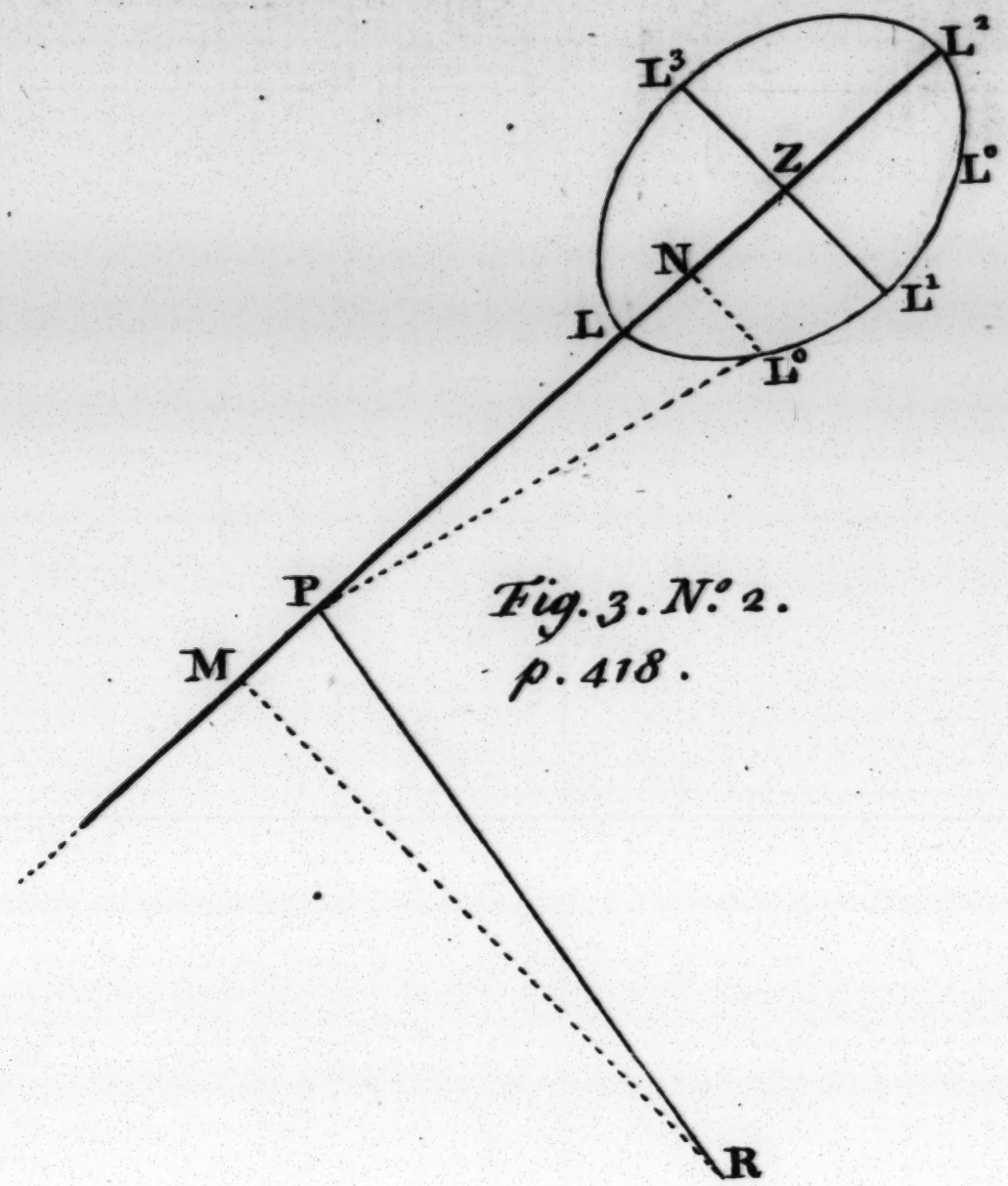
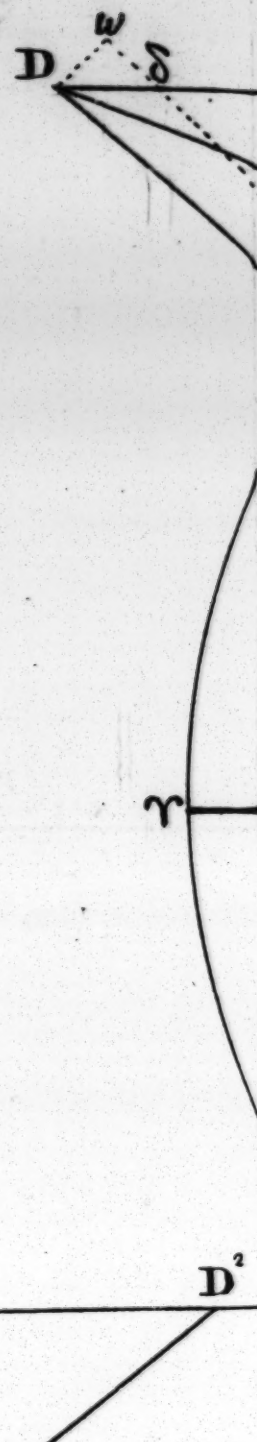


Fig. 3. N.° 2.
p. 418.



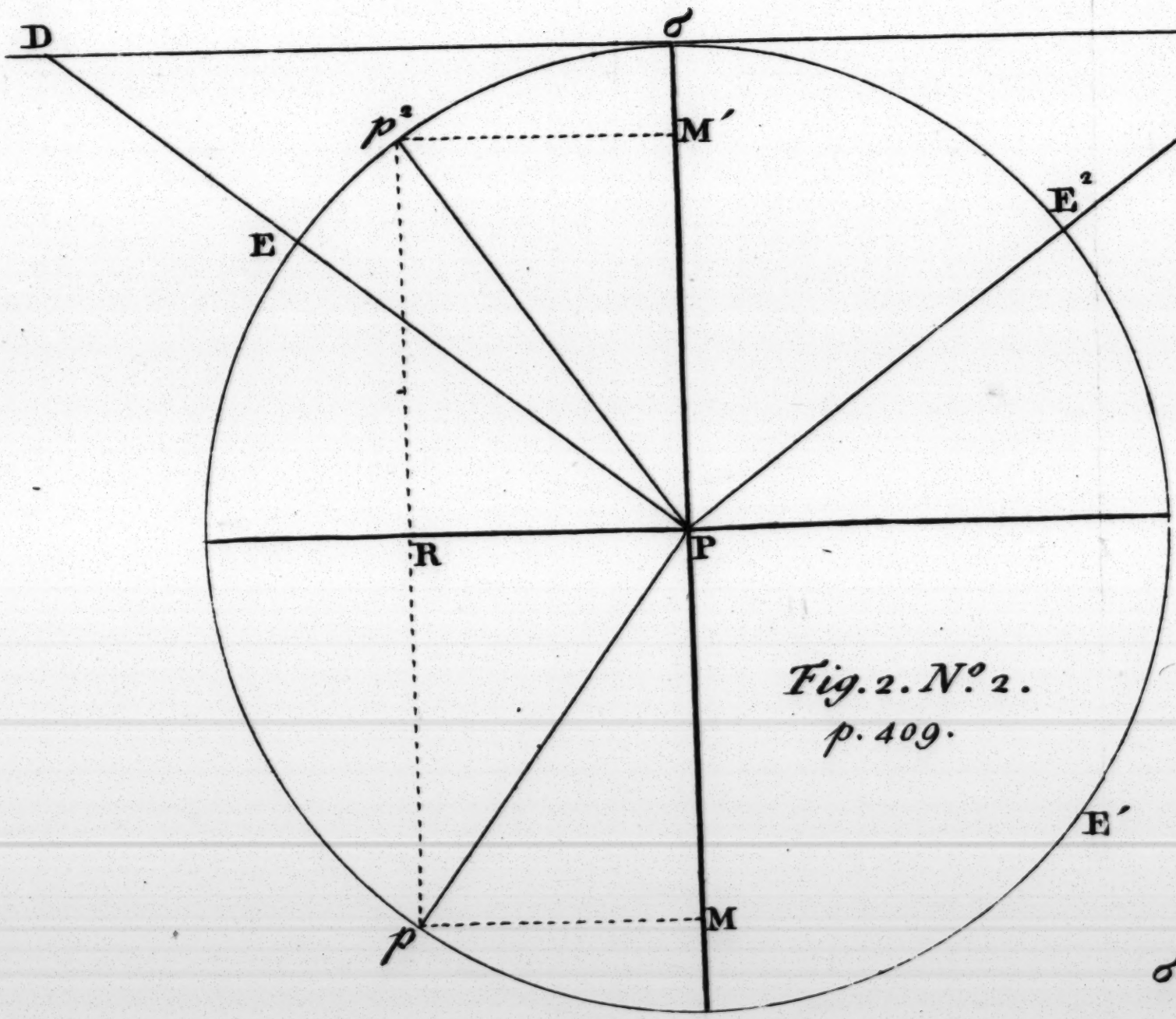
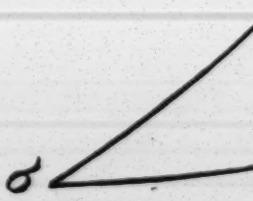


Fig. 2. N.° 2.
p. 409.



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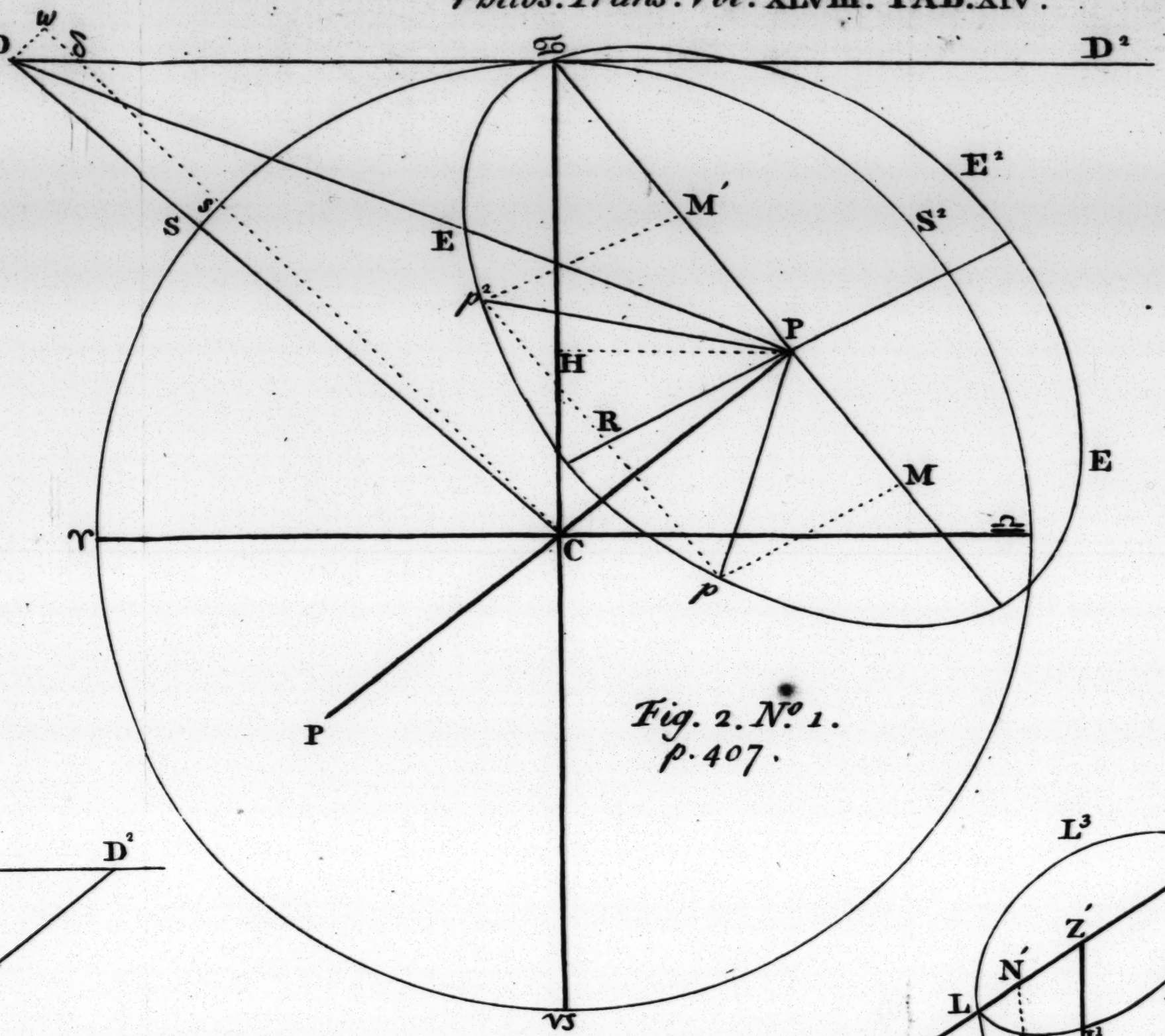


Fig. 2. N.° 1.
p. 407.

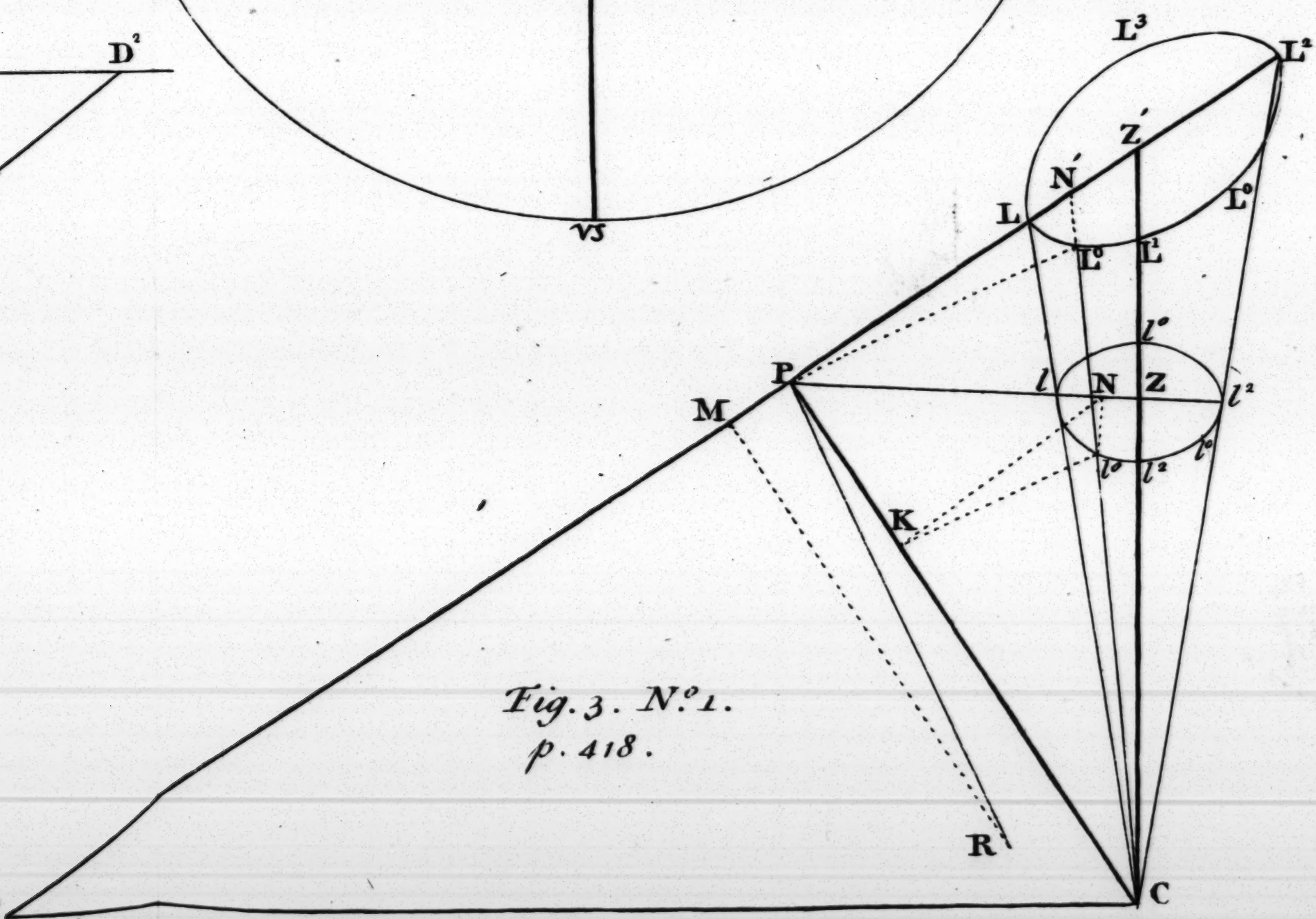


Fig. 3. N.° 1.
p. 418.

J. Mynde sculp.

ecliptic, as when the ſun was on the other ſide in S, but with a contrary ſign, or direction; and the ſum of the lines $P M$, from the ſolſtice ♋ to ♎, will be the ſame as the ſum of the lines $P M$, from ♈ to ♋, but with a contrary ſign: ſo that the angle of inclination of the earth's axe to the plane of the ecliptic is continually increaſing from ♈ to ♋, and then decreaſing from ♋ to ♎; and as the diminution is equal to the increaſe, the inclination becomes again the ſame in ♎ as it was in ♈, having undergone an oſcillation whoſe period is ſix months.

Whilſt the ſun paſſes from ♎ to ♑, there are the ſame motions of the pole, as whilſt it was paſſing from ♋ to ♎.

Thus is the motion of the pole determined at every inſtant, with reſpect to the ecliptic. Which was to be done.

Problem VI.

12. *To find the quantity of the preceſſion, or, which is the ſame, the arc of the parallel to the ecliptic run through by the pole of the earth* P *in the ſpace of time the ſun is paſſing from the equinox to the ſolſtice, or from the ſolſtice to the equinox; which may be taken for a quarter of the time of the ſun's revolution about the earth; the angle comprehended between the equinox and the enſuing ſolſtice differing but by an infinitely ſmall quantity from* 90 *degrees.*

Calling, as in *Prob.* I. the ſemi-axe $C P$, a; *fig.* 2. *n°* 1. *&* *n°* 2. the ſine of the angle $P C S$, or $P C D$, V; and its coſine, or the ſine of the ſun's declination, u:

Let $P\mathfrak{S}$, be b; $C\mathfrak{S}$, r; we ſhall have $CD = \frac{a}{u}$; $PD = \frac{a}{u}V$; the difference Ss of the arc ΥS, which is the ſun's diſtance from the node Υ, will be $\frac{rr \times d\overline{CD}}{\overline{CD} \times \mathfrak{S}D}$, as is found by theſe two proportions, $r : \overline{Ss} :: CD : D\omega = \frac{CD \times \overline{Ss}}{r}$, and $D\omega = \frac{CD \times \overline{Ss}}{r} : \measuredangle\omega =$ $d\overline{CD} : \overline{C\mathfrak{S}} = r : \overline{\mathfrak{S}D}$; whence we have $\overline{CD} \times \overline{Ss} \times \overline{\mathfrak{S}D} = rr \times d\overline{CD}$, and $\overline{Ss} = \frac{rr \times d\overline{CD}}{\overline{CD} \times \overline{\mathfrak{S}D}} = \frac{r\,du}{\sqrt{\frac{aa}{uu} - rr}}$ (putting inſtead of $\overline{CD}$ and $\overline{SD}$, their values $\frac{a}{u}$ and $\sqrt{\frac{aa}{uu} - rr}$) = to the ſpace run through by the ſun S in the ecliptic in an inſtant, $= 2\pi r \times \frac{dt}{T}$, calling T the time the ſun employs in running thro' the whole circumference $2\pi r$.

Whence we have $dt = \frac{T}{2\pi r} \times \frac{r\,du}{\sqrt{\frac{aa}{rr} - uu}} = \frac{T}{2\pi} \times \frac{du}{\sqrt{\frac{aa}{rr} - uu}}$

By

By *Art.* 8. we have $a\,P\,p = AV\,u \times a\,d\,t$; therefore, by *Prob.* V. the inſtantaneous preceſſion will be $AV\,u \times a\,d\,t \times \frac{\overline{PR}}{\overline{P\,p}}$, or $AV\,u \times a\,d\,t \times \frac{\overline{P\,\mathfrak{D}}}{\overline{P\,D}}$, the triangles $P\,p\,r$, $P\,D\,\mathfrak{S}$, being ſimilar, ſince the three ſides of the one are perpendicular to the three ſides of the other. Putting, therefore, inſtead of $\overline{P\,D}$, $\overline{P\,\mathfrak{S}}$, and $d\,t$, their values above found, we ſhall have the inſtantaneous preceſſion $= AV\,u \times \frac{a\,b}{\frac{a\,V}{u}} \times \frac{T}{2\pi} \sqrt{\frac{aa - uu}{rr}}$

$= Ab \times \frac{T}{2\pi} \times \frac{u\,u\,d\,u}{\sqrt{\frac{aa - uu}{rr}}}$. But $\int \frac{u\,u\,d\,u}{\sqrt{\frac{aa - uu}{rr}}} = -u\sqrt{\frac{aa - uu}{rr}} + \int du \sqrt{\frac{aa - uu}{rr}}$, or ſimply $= \int \frac{u\,u\,d\,u}{\sqrt{\frac{aa - uu}{rr}}}$

when $u = \frac{a}{r}$, as it happens when the ſun is in the ſolſtice $\mathfrak{S}$, and then $\int d\,u \sqrt{\frac{aa - uu}{rr}}$, which is the area of a quadrant of a circle whoſe radius is $\frac{a}{r}$, will be $\frac{a\pi}{2r} \times \frac{a}{2r} = \frac{aa\pi}{4rr}$.

Therefore the preceſſion, whilſt the ſun is paſſing from Υ to $\mathfrak{S}$, is $Ab \times \frac{T}{2\pi} \times \frac{aa\pi}{4rr} = Ab \times \frac{T}{2} \times \frac{aa}{4rr}$;

and calling CH, which is = the radius of the circle parallel to the ecliptic which the pole P runs thro', g; we ſhall have $g = \frac{aa}{r}$, and the preceſſion $Ab \times \frac{T}{2} \times \frac{aa}{4rr}$ will be likewiſe $Ab \times \frac{T}{2} \times \frac{g}{4r}$. Which was to be found.

And this alſo is the quantity of the preceſſion during the time the ſun paſſes from the ſolſtice to the equinox, or generally, during the time of each quadrant of the ſun's revolution; as was ſeen in *Prob.* V.

Remark.

13. The difference of the ſun's diſtance $S\Upsilon$ from the equinoctial point, is equal to the arc the ſun goes through in the ecliptic, when the node Υ is fix'd, as was ſuppos'd in the foregoing *Prob.* but when the node Υ moves in the ecliptic the contrary way to the ſun's motion, the difference of the ſun's diſtance from the node is equal to the arc gone through by the ſun, increaſed by the arc gone through by the node Υ.

Therefore always taking $S\,s$, *fig.* 2. *n°* 1. for the difference of the ſun's diſtance from the node, we ſhall have $\overline{S\,s}$ equal to the arc deſcribed by the ſun, more by the arc deſcribed by the node.

But, by the former *Prob.* we have ſeen, that the arc run through by the pole P in an inſtant dt, and in a parallel to the ecliptic, whereof g is the radius, is

$$AVu \times a\,dt \times \frac{\overline{P\,\mathfrak{S}}}{\overline{P\,D}} = AVu \times \frac{ab}{\frac{aV}{u}} \times dt = Abuudt.$$

The arc run through by the node Υ will be then

Abu

$Abuudt \times \frac{r}{S}$, ſince theſe arcs, which meaſure equal angles, are as their radii g and r.

We ſhall have, therefore, in the above *Problem*,

$$\overline{Ss}, \text{ or } \frac{r\,du}{\sqrt{\frac{aa-uu}{rr}}} = 2\pi r \times \frac{dt}{T} + Abuu \times \frac{r}{g}\,dt;$$

whence we get $dt = \frac{T\,du}{2\pi\sqrt{\frac{aa-uu}{rr}}} \times \frac{1}{1+Abuu \times \frac{T}{2\pi g}}$.

But $\frac{1}{1+Abuu \times \frac{T}{2\pi g}} = 1 - Abuu \times \frac{T}{2\pi g} +$, &c.

where the other terms may be neglected which contain the powers of A, becauſe A is a very ſmall fraction.

We ſhall have, therefore, $dt = \frac{T\,du}{2\pi\sqrt{\frac{aa-uu}{rr}}} \times \left(1 - Abuu \times \frac{T}{2\pi g}\right)$; and the inſtantaneous preceſſion will be $Ab \times \frac{T}{2\pi} \times \frac{uu\,du}{\sqrt{\frac{aa-uu}{rr}}} \times \left(1 - Abuu \times \frac{T}{2\pi g}\right)$ whoſe integral for the firſt term, in the ſolſticial point, where $u = \frac{a}{r}$, is $Ab \times \frac{T}{2\pi} \times \frac{aa\pi}{4rr}$, as was ſeen in the preceding *Problem*. But the integral of $u^4\,du$

$\frac{u^4 \, d\, u}{\sqrt{\frac{aa-uu}{rr}}} = u^3 \sqrt{\frac{aa-uu}{rr}} = \frac{3}{4} u \times \left(\frac{aa}{rr} - uu\right)^{\frac{1}{2}}$

$+ \frac{3}{4} \frac{aa}{rr} \int d\, u \sqrt{\frac{aa-uu}{rr}}$, or simply, $= \frac{3}{4} \frac{aa}{rr} \int d\, u \sqrt{\frac{aa-uu}{rr}}$, when we suppose, after the integration, $\frac{a}{r} = u$; and $= \frac{3}{4} \frac{aa}{rr} \times \frac{a\pi}{2r} = \frac{3}{4} \frac{aa}{rr} \times \frac{aa\pi}{4rr}$. Therefore the precession from the sun's departure from the equinox till his arrival at the solstice, is $Ab \times \frac{T}{2} \times \frac{aa}{4rr} \times \left(1 - Ab \times \frac{T}{2\pi g} \times \frac{3}{4} \frac{aa}{rr}\right) = \left(g \text{ being} = \frac{aa}{r}\right) Ab \times \frac{T}{2r} \times \frac{g}{4} \times \left(1 - Ab \times \frac{T}{2\pi r} \times \frac{3}{4}\right)$.

When the motion of the node is not very sensible, it will suffice to make use of the *formula* of the preceding *Problem*, which is the first term of this: But when that motion is sensible, which is the case of the moon's node, when it is to be determined for a quarter of the revolution of the sun, it will be requisite to employ this *formula*, which contains two terms of the series. Nay, it would be very easy to take a greater number of terms, so as to neglect nothing; but this would be absolutely useless, because of the quick converging of the series.

PRO-

Problem VII.

14. *To find the alteration of the inclination of the earth's axe to the ecliptic, during the ſun's paſſage from the equinox to the ſolſtice, or from the ſolſtice to the next equinox; which is the ſame, only affected with a contrary ſign, as has been ſeen in* Prob. V.

By *Art.* 8. we have $aPp = AVu \times adt$; and by *Prob.* V. the inſtantaneous variation of the inclination of the earth's axe to the ecliptic is $AVu \times adt \times \frac{PM}{Pp} = AVu \times adt \times \frac{\overline{\mathfrak{S}D}}{\overline{PD}}$, becauſe of the ſimilar triangles $PpM, PD\mathfrak{S}$, $=$ (taking the denominations of *Prob.* VI.) $AVu \times a \times \frac{T}{2\pi} \times \frac{r \times d\overline{CD}}{\overline{CD} \times \overline{\mathfrak{S}D}} \times \frac{\overline{\mathfrak{S}D}}{\overline{PD}} = AVu \times \frac{aT}{2\pi} \times \frac{r\,d\overline{CD}}{\overline{CD} \times \overline{PD}} = AVu \times \frac{aT}{2\pi} \times \frac{r \times \frac{du}{uu}}{\frac{ar}{u} \times \frac{a}{u}} = AV \times \frac{T}{2\pi} \times u\,du$, whoſe integral is $\frac{1}{2} Ar \times \frac{T}{2\pi} \times uu$; and when the ſun is at the point $\mathfrak{S}$, we have $1 : u :: r : a$, and $a = ru$; therefore the preceding integral is then $\frac{1}{2} A \times \frac{T}{2\pi r} \times aa$. Which was to be found.

Remark.

15. When the motion of the node is not inſenſible, you are to take the value of dt of the *Remark* that follows

follows *Art.* 12. which gives $dt = \frac{T}{2\pi} \times \frac{r \times d\overline{CD}}{\overline{CD} \times \overline{\mathfrak{S}D}} \times (1 - Ab \times \frac{T}{2\pi g} \times uu)$, and the inſtantaneous variation of the inclination of the earth's axe to the ecliptic will be $Ar \times \frac{T}{2\pi} \times u\,du \times (1 - Ab \times \frac{T}{2\pi g} \times uu)$, whoſe integral, when the ſun arrives at the ſolſtice, where we have $ru = a$, becomes $\frac{1}{2} Aaa \times \frac{T}{2\pi r} - \frac{1}{4} \frac{Aa^4}{r^2} \times \frac{T}{2\pi r} \times Ab \times \frac{T}{2\pi g} = \left(\text{becauſe } g = \frac{aa}{r}\right) \frac{1}{2} Aaa \times \frac{T}{2\pi r} \times (1 - \frac{1}{2} Ab \times \frac{T}{2\pi r})$; and this is the *formula* to be made uſe of when the motion of the nodes is ſenſible.

SECT II.

Of the motion of the pole of the equator of the earth cauſed by the action of the moon.

16. Whatever has been ſaid in *Sect.* I. of the preceſſion, and of the oſcillation of the earth's axe on the plane of the ecliptic, through the action of the ſun, is equally applicable to the action of the moon; it being requiſite only to ſubſtitute every-where the moon inſtead of the ſun; L inſtead of S, and λ inſtead of s; the time which the moon employs in completing her revolution about the earth, inſtead of the time the ſun employs in performing his

his revolution about the earth, or M inſtead of T: Laſtly, the lunar orbit, inſtead of the ecliptic. Likewiſe, inſtead of A, which (*Art.* 8.) was equal to $\frac{3S}{s^3m}$, we muſt put $B = \frac{3L}{\lambda^3 m} \times \frac{a\,a}{1-2a}$.

17. We ſhall find then, by *Art.* 12. that the arc deſcribed by the pole P of the earth parallel to the plane of the lunar orbit, whilſt the moon makes one quarter of a revolution about the earth, is $Bb \times \frac{g}{2r} \times \frac{M}{4}$, which gives the arc deſcribed by the mean motion in an inſtant, dt, equal to $Bb \times \frac{g}{2r} \times dt = Bb \times \frac{a\,a}{2\,r\,r} \times dt$; putting, inſtead of g, its value $\frac{a\,a}{r}$.

18. We ſhall find alſo, by *Art.* 14. that the variation of the inclination of the earth's axe to the plane of the lunar orbit, when the moon is at its point of ſtation 90 degrees diſtant from the node of the equator with the lunar orbit, is $\frac{1}{2} B \times \frac{M}{2\pi r} \times a\,a$, whereby the angle of inclination of the earth's axe to the plane of the lunar orbit is increaſed when the moon is at its point of ſtation.

19. We therefore ſhould have nothing to add, if the plane of the lunar orbit were the ſame as that of the ecliptic, or if it always retained the ſame poſition: But as this plane continually varies its poſition, and its pole deſcribes a ſmall circle about the pole of the ecliptic, from whence it is always about 5 degrees diſtant; it ſtill remains that we examine what is the

preceſſion cauſed by the moon, with reſpect to the ecliptic; which may be done by reſolving the motion of the earth's pole, parallel to the plane of the lunar orbit, into two motions, one whereof is parallel to the plane of the ecliptic, and the other perpendicular thereto, and which varies the inclination of the earth's axe to the ecliptic. This is what we ſhall determine in the following *Problem*.

Problem VIII.

20. *Having given, the motion of the pole* P *of the earth parallel to the plane of the lunar orbit, to find its motion parallel to the plane of the ecliptic.*

* *C P, fig.* 3. *n°* 1. *&* *n°* 2. is the ſemi-axe of the earth. The plane of the figure is the plane of the ſolſticial colure; *C Z* is the axe of the ecliptic, conſequently perpendicular to *C* ♋; ♋ the ſolſticial point, and *P C* ♋ the ſolſticial angle: *P Z* is parallel to *C* ♋, and perpendicular to *C Z*; and it is equal to the line *C H* of *fig.* 2. The circumferencc l, l', l^2, l^3, l is the circumference deſcribed by the pole, *l*, of the lunar orbit; L, L', L^2, L^3 is the ſection of the cone $C\,l, l'\,l^2\,l^3\,l$ prolonged, by the plane that is tangential at the pole of the earth, *P*.

Let l° be any place of the pole of the moon's orbit. If upon the point *P* be erected *P R*, perpendicular to the plane $P\,C\,l^\circ$, this perpendicular will be the direction of the motion of the earth's pole, parallel to

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* The lines which are in the plane which touches the earth's pole *l*, are ſeparately repreſented in *fig.* 2. *n°* 3.

the plane of the lunar orbit, the plane PCl° being perpendicular to that plane, PCl° being the axe thereof.

Letting fall from the point R the perpendicular RM upon the line $\mathfrak{S} PZ$, the motion of the pole of the earth according to PR may be reſolved into two; the one according to PM, the other parallel and equal to RM; and this latter expreſſes the preceſſion with regard to the ecliptic, and the former gives the alteration of the inclination of the earth's axe to the plane of the ecliptic.

The motion of the pole repreſented by MR, which gives the preceſſion with regard to the ecliptic, is always on the ſame ſide during the entire revolution of the pole of the lunar orbit, and is the ſame on either ſide the point l at the like diſtance from it; the motion according to PR being then the ſame, and always on the ſame ſide, as has been explained in *Prob.* V.

* The motion according to PM is the ſame on either ſide of the point l at the ſame diſtance from it, but with a contrary ſign, that is, in an oppoſite direction. For it is manifeſt, that if another point, as l°, be taken on the other ſide, and at an equal diſtance from the point l, the point M, where the perpendicular RM falls, will be on the other ſide with regard to P, than the point M, ſince the angle RPZ', which is here obtuſe, will then be acute, and leſs than the right angle of an angle equal to $L^{\circ} PZ'$.

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* The point l° placed on the other ſide of the point l, that is, between l and l^{3}, is not marked in the *fig.* nor the point M' where the perpendicular KM' falls; for the ſake of not embarraſſing the *fig.* with too many lines, it being eaſy to conceive.

For this reaſon, the motion according to PM muſt produce an oſcillation of the earth's axis, whoſe period is the time of an entire revolution of the pole of the lunar orbit about the pole of the ecliptic.

Problem IX.

21. *To find the quantity of the preceſſion cauſed by the moon with regard to the ecliptic; that is, the arc parallel to the ecliptic, run through by the earth's pole* P, *during the time of a ſemi-revolution* $l\,l^{1}\,l^{2}$ *of the pole* l *of the lunar orbit.*

Let there be drawn the lines NK, $l^{\circ}K$ perpendicular to the axe of the earth CP, and parallel to the lines NP, $l^{\circ}\mathcal{P}$, *fig.* 3. *n*o 1. *&* *n*° 2.

The triangles PRM, $\mathcal{P}NL^{\circ}$, KNl° are right-angled and ſimilar; for the three ſides of the firſt are perpendicular to the three ſides of the ſecond, and the three ſides of the ſecond are parallel to the three ſides of the third.

Still retaining the foregoing denominations, let $zl=x$; $ZN=z$; $\mathcal{P}Z=g$, as in *fig.* 2. the line CH; $CZ=f$; Cl or $Cl^{\circ}=C$.

We ſhall have $Nl^{\circ}=\sqrt{xx-zz}$; $NP=g-z$; $NK=\frac{f}{a}\times\overline{g-z}$, or $\frac{f}{a}\times\overline{g+z}$, when the point l° lies on the other ſide of l^{1}; $PR=\frac{g}{a}\times\overline{g-z}$, or $\frac{g}{a}\times\overline{g+z}$ when the point l° is on the other ſide of l^{1}. The triangle $Cl^{\circ}K$ is here ſimilar to the triangle whoſe three ſides are repreſented by the letters a, b, r, ſince

ſince $\frac{b}{r}$ expreſſes here the ſine of the angle of inclination of the earth's axe to the plane of the lunar orbit, and $\frac{a}{r}$ is its coſine; we ſhall have then $l^\circ K = \frac{ac}{r}$; $CK = \frac{bc}{r} = C\mathcal{P} - \mathcal{P}K = a - \frac{g}{a} \times \overline{g-z} = \frac{aa-gg+gz}{a} = \frac{ff+gz}{a}$, or $\frac{ff-gz}{a}$, when the point l° is on the other ſide with reſpect to l^1; $\frac{RM}{PR} = \frac{NK}{l^\circ K} = \frac{\frac{f}{a} \times \overline{g-z}}{\frac{ac}{r}}$, or $\frac{\frac{f}{a} \times \overline{g+z}}{\frac{ac}{r}}$ for the other ſide of l^1; $\frac{\overline{PM}}{PR} = \frac{Nl^\circ}{l^\circ K} = \sqrt{\frac{xx-zz}{ac}}$: Laſtly, the difference of the arc $l^1\, l^\circ = du = \frac{x\,dz}{\sqrt{xx-zz}} = \frac{2\pi x}{nT} \times dt$, calling nT the time which the pole of the lunar orbit employs in performing its revolution in the circumference $2\pi x$, whence we get $dt = \frac{nT}{2\pi} \times \frac{dz}{\sqrt{xx-zz}}$.

The ſpace run through in an inſtant dt by the pole P of the earth parallel to the plane of the lunar orbit being

being by *Art.* 17. $= Bb \times \frac{aa}{2r} \times dt$; and by *Prob.* VIII. the ſpace run through parallel to the plane of the ecliptic, will be $Bb \times \frac{aa}{2rr} \times dt \times \frac{x\,I}{P\,x} =$ (by the preceding denominations) $Bb \times \frac{aa}{2rr} \times \frac{nT}{2\pi} \times \frac{dz}{\sqrt{xx - zz}}$

$\times \frac{\frac{f}{a} \times \overline{g - z}}{\frac{ac}{r}} = B \times \frac{b}{r} \times \frac{nT}{4\pi} \times \frac{dz}{\sqrt{xx - zz}} \times \frac{f \times \overline{g - z}}{c}$;

and putting inſtead of $\frac{b}{r}$, its value, which was found to be $\frac{ff+gz}{ac}$, we ſhall have $B \times \frac{nT}{4\pi} \times \frac{f}{acc} \times \overline{ff+gz}$ $\times \overline{g-z} \times \frac{dz}{\sqrt{xx - zz}}$; and when the point l° is on the other ſide with regard to $l^{\prime}$, we ſhall have $B \times \frac{nT}{4\pi}$ $\times \frac{f}{acc} \times \overline{ff - gz} \times \overline{g+z} \times \frac{dz}{\sqrt{xx - zz}}$. Therefore always taking the preceſſion for the two points together placed on either ſide of $l^{\prime}$, and at an equal diſtance from it, we ſhall have, by joining the two values now found, after making reduction, $B \times \frac{nT}{4\pi} \times \frac{f}{aa} \times$ $\overline{2gff - 2gzz} \times \frac{dz}{\sqrt{xx - zz}}$, or $B \times \frac{nT}{2\pi} \times \frac{fg}{aa} \times$ $\overline{ff - zz} \times \frac{dz}{\sqrt{xx - zz}}$.

But

But the integral of $ff \times \frac{dz}{\sqrt{xx - zz}} = \frac{ff}{x} \times \frac{dz}{\sqrt{xx - zz}}$ is $\frac{ff}{x} \times \frac{x\pi}{2} = \frac{ff\pi}{2}$, when we take all the points l°, from l^{1} to l; and the integral of $\frac{zz\,dz}{\sqrt{xx - zz}}$, which is $-z\sqrt{xx - zz} + \int dz \sqrt{xx - zz}$; when, after the integration, we make $z = x$, becomes $\int dz \sqrt{xx - zz}$, or the area of the quadrant $l^{1}\,z\,l$, which is $= \frac{x\pi}{2} \times \frac{x}{2} = \frac{xx\pi}{4}$.

Therefore the integral of $B \times \frac{nT}{2\pi} \times \frac{fg}{acc} \times \sqrt{ff - zz} \times \frac{dz}{\sqrt{xx - zz}}$ is $= B \times \frac{nT}{2\pi} \times \frac{fg}{acc} \times \left(\frac{ff\pi}{2} - \frac{xx\pi}{4}\right) = B \times \frac{nT}{2} \times \frac{fg}{acc} \times \left(\frac{ff}{2} - \frac{xx}{4}\right) = B \times \frac{nT}{2} \times \frac{gf^{3}}{2acc} \times \left(1 - \frac{xx}{2ff}\right)$.

This is the ſum of all the inſtantaneous preceſſions for all places of the lunar orbit from l to l^{2}, becauſe we have always taken two points together placed on either ſide of l^{1}.

And this is the quantity of the preceſſion with regard to the ecliptic, during the time of a ſemi-revolution of the lunar orbit. Which was to be found.

PRO-

Problem X.

22. *To find the quantity of the nutation, or of the variation of the inclination of the earth's axe to the ecliptic, caused by the moon during the semi-revolution* $l\,l^1\,l^2$ *of the pole of the lunar orbit.*

The space run through in an instant dt by the pole of the earth, parallel to the plane of the lunar orbit, being, by *Art.* 17. $= Bb \times \frac{aa}{2rr} \times dt$, the instantaneous nutation for any one place l^o of the lunar orbit will be, by *Prob.* VIII. $= Bb \times \frac{aa}{2rr} \times dt \times \frac{PM}{PR}$ = (by the denomination of the preceding *Problem*) $Bb \times \frac{aa}{2rr} \times \frac{nT}{2\pi} \times \frac{dz}{\sqrt{xx - zz}} \times \frac{\sqrt{xx - zz}}{\frac{ac}{r}} = B \times \frac{b}{r} \times \frac{aa}{2} \times \frac{nT}{2\pi} \times \frac{dz}{\sqrt{xx - zz}}$. Putting instead of $\frac{b}{r}$ its value, which was found to be $\frac{ff + gz}{ac}$, we shall have $B \times \frac{nT}{2\pi \times 2cc} \times \sqrt{ff + gz} \times dz$; and when the point l^o is on the other side with regard to l^1, we shall have $B \times \frac{nT}{2\pi \times 2cc} \times \sqrt{ff - gz} \times dz$.

Taking therefore the nutation for the two points together, l^o, placed on either side of l^1, we have

$B \times$

$B \times \frac{nT}{2\pi \times 2cc} \times 2ffdz$, whoſe integral, when z is $= x$, is $B \times \frac{nT}{2\pi \times 2cc} \times 2ff\,x = B \times \frac{nT}{2\pi} \times \frac{ff}{cc} \times x$.

This is the quantity of the nutation cauſed by the moon during the ſemi-revolution $l\,l'\,l''$ of the pole of the lunar orbit. Which was to be found.

COROLLARY.

23. By *Prob.* IX. the quantity of the preceſſion cauſed by the moon, with regard to the ecliptic, is, during the ſpace of a ſemi-revolution $l\,l'\,l''$ of the pole of the lunar orbit, $B \times \frac{nT}{2} \times \frac{gf^3}{2acc} \times \left(1 - \frac{1}{2} \times \frac{xx}{ff}\right)$.

By the preceding *Problem*, the quantity of the nutation during the ſame time of a ſemi-revolution of the pole of the lunar orbit $l\,l'\,l''$, cauſed alſo by the moon, is $B \times \frac{nT}{2\pi} \times \frac{ff}{cc} \times x$.

Therefore the preceſſion, during the time of a ſemi-revolution of the pole of the lunar orbit, is to the nutation, during the ſame time,

As $\frac{gf}{2a} \times \left(1 - \frac{1}{2}\,\frac{xx}{ff}\right)$, is to $\frac{x}{\pi}$; or as $gf\pi \times \left(1 - \frac{1}{2}\,\frac{xx}{ff}\right)$ is to $2ax$.

But the preceſſion is performed in a parallel to the ecliptic, whereof g is the radius; and the nutation is performed in the plane of the ſolſticial colure, and in a circle whereof a is the radius: Di-

viding, therefore, each circular arc by its radius, we ſhall have the quantity of the angle: And

The angle of the preceſſion, during the time of a ſemi-revolution of $l\,l'\,l''$ of the pole of the lunar orbit, will be to the angle of the nutation, in the ſame time,

As $f\,\pi \times \left(1 - \frac{1}{2}\frac{xx}{ff}\right)$ is to $2\,x$, or as $\pi\left(1 - \frac{1}{2}\frac{xx}{ff}\right)$ is to $\frac{2\,x}{f}$.

SCHOLIUM.

24. Whatever has been ſaid as to the moon and the ſun, may be equally applicable to all the other planets; but as the effect of the action of the reſt of the planets upon the earth is not ſenſible, we ſhall not take it into conſideration.

SECT. III.

Of the motion of the pole of a ring, cauſed by the action of the ſun: or of the motion of the pole of the moon's orbit.

25. We have ſeen, in *Art.* 9. that if, inſtead of the terreſtrial ſpheroid, we were to ſuppoſe a ſimple ring placed at the circumference of the equator, the motion of the pole of ſuch ring would be found by the ſame *formula* as that for the motion of the pole of the earth; ſuppoſing only, in that *formula*, $\frac{a}{1-2a} = 1$.

By

By this means we may apply all the *formulæ* which have been found in *Sect.* I. to the motion of the nodes of this ring, and to the alteration of the inclination of its axe to the plane of the ecliptic.

But by the *Remark* on *Art.* 6. the motion of the plane of this ring is the ſame, whether the ring be entire, or there be only a ſingle point which circulates in the ring's circumference: Whence it follows, that

Theſe ſame *formulæ* do likewiſe give the motion of the pole of the orbit of a moon or ſatellite; the motion of the node of the orbit of ſuch moon in the ecliptic; and the variation of the inclination of its axe of the orbit to the plane of the ecliptic: Obſerving to put the time of the revolution of this moon about the ſun, inſtead of the time of the earth's revolution about the ſun, and the motion of the moon in its orbit inſtead of the motion of a point of the equator.

Remark.

It may be obſerved, that although the motion of the pole of a ring be the ſame as that of a moon, during the time of the revolution of the ring, or of the moon, which is the ſame; yet there are ſome particular motions which take place when there is only one moon revolving in the circumference of the ring, and which ceaſe to exiſt when it is an entire ring that revolves.

For example; in *Art.* 2. the force, according to $L\gamma$, *fig.* 1. n° 2. of the point L, is deſtroyed by an equal and directly oppoſite force of another point λ placed on the other ſide, and at the ſame diſtance from the point E, as the point L, when the ring is entire;

but when there is but one point, as L, or, which is the ſame, one moon, which revolves in the circumference of the ring; this motion ſubſiſts, and diſturbs the motion of the moon in the plane of its orbit.

In like manner the force of the point L parallel to γC, or according to $c' L$, is deſtroyed, in a ring, by the force of another equal point λ' placed on the other ſide with regard to $\mathcal{Q}$, and at the ſame diſtance as the point L. But this force ſubſiſts in the caſe where there is but one ſingle point L, or one moon, and this motion diſorders, beſides, the motion of the moon in the plane of its orbit.

Laſtly, If the ring, which circulates about the centre C, be elliptical, the motion of every point of the ring in the orbit is proportional to its diſtance from the centre; becauſe, on account of the continuity of the ring, all the points complete their revolution in the ſame time, and have equal angular motions; but when it is but one ſingle moon that revolves, in an elliptic orbit, it deſcribes areas proportional to the times, and has an angular motion ſo much the greater, as it approaches nearer the centre; which is the contrary of the motion of the points of the elliptic ring, the more diſtant whereof from the centre, have the moſt motion. And it ariſes from hence, that the motion of the pole of the lunar orbit is much the ſame as that of the ring, taking it during the entire revolution; but that there is a ſmall variation, or inequality, in the inſtantaneous motion of the pole of a moon; whereas this inſtantaneous motion is conſtant through every inſtant of the revolution of the ring.

I ſhall

I ſhall ſpend no more time on the ring, with reſpect to thoſe motions which have nothing to do with the ſubject in queſtion; but they ſhall be thoroughly diſcuſſed, and applied, in a ſecond memoir, which I ſhall publiſh, on the particular motions of the planets.

SECT. IV.

Containing the application of the formulæ *of the preceding Sections.*

26. The motion of the ſun, or, to ſpeak more exactly, of the earth, in the ecliptic, is $\frac{2\pi s}{T}$, which is the ſpace divided by the time.

27. The earth's central force will therefore be $\left(\frac{2\pi s}{T}\right)^2 \times \frac{1}{2s}$, the central force being equal to the ſquare of the motion divided by the diameter of the circumference in which the body moves; and the earth's central force being the force $\frac{S}{s\,s}$ wherewith it is attracted towards the ſun; we ſhall have $\frac{S}{s\,s} = \left(\frac{2\pi s}{T}\right)^2 \times \frac{1}{2s} = \frac{2\pi\pi s}{TT}$; whence we get $\frac{S}{s^3} = \frac{2\pi\pi}{TT}$.

28. The motion of a point of the equator whoſe radius is a, will be, calling the time of its revolution t, $= \frac{2A\pi}{t}$, which is the ſpace divided by the time.

We have therefore $m = \frac{2A\pi}{t}$.

29.

29. By *Art.* 8. $A = \frac{3S}{s^3 m} \times \frac{a\,a}{1-2a} =$ (by *Articles* 27. & 28.) $3 \times \frac{2\pi\pi}{TT \times 2a\pi} \times \frac{a\,a}{1-2a} = \frac{3t}{TT} \times \pi \times \frac{a}{1-2a}$ $=$ (by *Art.* 31. following) $\frac{3t}{TT} \times \pi \times \frac{1}{176}$.

30. Putting q for the ratio of the lunar force $\frac{L}{\lambda^3}$, to the ſolar force $\frac{S}{s^3}$, we ſhall have $\frac{L}{\lambda^3} = \frac{S}{s^3} \times q$; and B, which (by *Art.* 16.) is $= \frac{3L}{\lambda^3 m} \times \frac{a\,a}{1-2a}$ will alſo be $= \frac{3S}{s^3 m} \times q \times \frac{a\,a}{1-2a} =$ (by *Art.* 29.) $\frac{3qt}{TT} \times \pi \times \frac{1}{176}$.

31. Suppoſing $a = \frac{1}{178}$, which is conformable to the obſervations of the gentlemen of the *Pariſian* academy, we ſhall have $\frac{a}{1-2a} = \frac{1}{178 \times 1 - \frac{2}{178}} = \frac{1}{176}$.

Problem XI.

32. *To find the preceſſion cauſed by the ſun during the time between the equinox and the ſolſtice; that is to ſay, in a quarter of a year, or three months.*

By *Art.* 12. the preceſſion, during this time, is $Ab \times \frac{T}{2} \times \frac{g}{4r}$. Putting, inſtead of A, the value found

found in *Art.* 29. we ſhall have $\frac{3t}{TT} \times \pi \times \frac{1}{176} \times \frac{bT}{2} \times \frac{g}{4r} = \frac{3t}{T} \times \frac{1}{176} \times \frac{b}{r} \times \frac{\pi g}{8}$.

But $\frac{b}{r}$ is the ratio of the ſine of the inclination of the axe of the earth to the ecliptic, to the radius, or of the ſine of 66 degrees 32 minutes to the radius, $= \frac{9172919}{10000600}$; and πg is the ſemi-circumference of the parallel to the ecliptic wherein the pole of the earth moves, and, conſequently, equal to 180 deg.

t, which is the time of the revolution of a point of the equator, is equal to one day; and T equal to $365\frac{1}{4}$ days; we ſhall have, then, by ſubſtituting all the values in the preceding *formulæ*, $\frac{3}{365\frac{1}{4}} \times \frac{9172919}{10000000} \times \frac{1}{176} \times \frac{180}{8}$ degrees. This is the preceſſion produced by the ſun during the ſpace of 3 months.

33. Multiplying by 4, we ſhall have the annual preceſſion cauſed by the ſun $= \frac{3}{365\frac{1}{4}} \times \frac{9172919}{10000000} \times \frac{1}{176} \times \frac{180}{2}$ degrees, $= \frac{3}{365\frac{1}{4}} \times \frac{9172919}{10000000} \times \frac{45}{88}$ degrees, = (after due reduction) $13''.52'''.11''''$.

Problem XII.

34. *To find the preceſſion cauſed by the moon, during the time of a ſemi-revolution of the pole of the lunar orbit about the pole of the ecliptic; that is, in about* $9\frac{1}{3}$ *years, or* $9\frac{1}{3} \times T$, *as* $T = 1$ *year.*

By *Art.* 21. we have found this preceſſion $= B \times \frac{nT}{2} \times \frac{gf^3}{2acc} \times \left(1 - \frac{1}{2} \times \frac{xx}{ff}\right)$.

By *Art.* 30. $B = \frac{3qt}{TT} \times \pi \times \frac{1}{176}$: Subſtituting this value in the preceding *formula*, it becomes $\frac{3qt}{T} \times \frac{n}{2} \times \frac{f}{2a} \times \frac{ff}{cc} \times \frac{1}{176} \pi g \times \left(1 - \frac{1}{2}\frac{xx}{ff}\right)$.

But $\frac{f}{a}$ is the ratio of the ſine of 66 deg. 32 min. to the radius, $= \frac{9172919}{10000000}$.

$\frac{f}{c}$ is the ratio of the ſine of 85 deg. to the radius, the angle $l\,c\,z$, whereby the pole of the lunar orbit is diſtant from the pole of the ecliptic, being, at all times, about 5 degrees. Therefore $\frac{f}{c} = \frac{9961947}{10000000}$.

$\frac{x}{f}$ is the ratio of the ſine of 5 deg. to the ſine of 85 degrees, $= \frac{871557}{9961947}$.

Therefore

Therefore ſubſtituting theſe values, and thoſe of t, T, and πg found in the preceding *Problem*, we ſhall find that the preceſſion, during a ſemi-revolution of the lunar pole, which is $\frac{nT}{2}$, by *Art.* 21. becomes

$$= q \times \frac{n}{2} \times \frac{3}{365\frac{1}{4}} \times \frac{9172919}{10000000 \times 2} \times \left(\frac{9961947}{10000000}\right)^2 \times \frac{1}{176} \times 180 \text{ deg.} \times \left(1 - \tfrac{1}{2} \times \left(\frac{871557}{9961947}\right)^2\right).$$

This is the preceſſion cauſed by the moon during the time of a ſemi-revolution of the pole of the lunar orbit, $\frac{nT}{2} = 9\frac{1}{3} \times T$.

35. Therefore the mean preceſſion cauſed by the moon during the time T, which is the mean annual preceſſion cauſed by the moon, will be, by dividing the preceding *formula* by $\frac{n}{2}$, $= q \times \frac{3}{365\frac{1}{4}} \times \frac{9172919}{10000000 \times 2} \times \left(\frac{9961947}{10000200}\right)^2 \times \frac{180 \text{ deg.}}{176} \times \left(1 - \tfrac{1}{2} \times \left(\frac{871557}{9961947}\right)^2\right)$ where it only remains to determine the value of q, which, by *Art.* 30. is the ratio of the lunar force to the ſolar force; and making this ratio $= \frac{5}{2}$, conformable to the obſervations of the tides related by Mr. *Daniel Bernoulli*, we ſhall have for the mean annual preceſſion cauſed by the moon, $\frac{5}{2} \times \frac{3}{365\frac{1}{4}} \times \frac{9172919}{10000000 \times 2} \times \left(\frac{9961947}{10000000}\right)^2 \times \frac{180 \text{ deg.}}{176} \times \left(1 - \tfrac{1}{2} \times \left(\frac{871557}{9961947}\right)^2\right)$ $=$ (after due reduction) $34''.\ 16'''.\ 51''''.$

Problem XIII.

36. *To find the nutation of the earth's axe caused by the moon, during the time of a semi-revolution of the pole of the moon's orbit, that is, in* $9\frac{1}{3}$ *years.*

By *Art.* 23. $\pi \times \left(1 - \frac{1}{2} \times \frac{x\,x}{f\,f}\right)$ is to $\frac{2\,x}{f}$, as the angle of the precession, found in the preceding *Problem* for the time $\frac{n\,T}{2}$ of the semi-revolution of the pole of the lunar orbit, is to the nutation during the same time. By it, therefore, may be found the nutation, having the quantity of the precession of the preceding *Prob.* or, reciprocally, knowing the nutation during the time of a semi-revolution of the pole of the lunar orbit, the precession will be known to a geometrical certainty, for the same time. The ratio, in numbers, is $\frac{1095798}{61236}$.

37. But we may also know the nutation during the time of a semi-revolution of the pole of the lunar orbit, by the *formula* of *Art.* 22. which is $B \times \frac{n\,T}{2} \times \frac{f\,f}{c\,c} \times x$; which, without alteration, may be multiplied by $\frac{a\,\pi}{a\,\pi}$, or by $\frac{a\,\pi}{a \times \frac{22}{7}}$; since π, which is the ratio of the circumference to the diameter, is $\frac{22}{7}$, and we shall have

have the *formula* $B \times \frac{nT}{2} \times \frac{ff}{cc} \times x \times \frac{a\pi}{a \times \frac{22}{7}} = B \times \frac{nT}{2} \times \frac{f}{a} \times \frac{f}{c} \times \frac{n}{c} \times \frac{7}{22} \times a\pi$; which, by ſubſtituting the values of the preceding *Problem*, and 180 degrees inſtead of $a\pi$, which is the ſemi-circumference of the circle wherein the nutation is performed, becomes $\frac{3qt}{T} \times \frac{n}{2} \times \frac{9172919}{10000000} \times \frac{9961947}{10000000} \times \frac{871557}{10000000} \times \frac{7}{22} \times \frac{180}{176}$ deg. $= \frac{3}{365\frac{1}{4}} \times \frac{5}{2} \times 9\frac{1}{3} \times \frac{9172919}{10000000} \times \frac{9961947}{10000000} \times \frac{871557}{10000000} \times \frac{7}{22} \times \frac{180}{176}$ deg. equal, after reduction, to $17''.51'''.14''''$.

Problem XIV.

38. *To find the difference of the inclination of the earth's axe, between the point of the equinox and that of the ſolſtice, cauſed by the action of the ſun.*

By *Art.* 14. the *formula* is $\frac{1}{2} A \times \frac{T}{2\pi r} \times a\,a$.

Putting, inſtead of A, its value of *Art.* 29. inſtead of $\frac{a}{r}$, which is the ratio of the ſine of the obliquity of the ecliptic to the radius $\frac{3982155}{10000000}$, we ſhall have $\frac{1}{2} \times \frac{3}{365\frac{1}{4}} \times \frac{1}{2 \times 176} \times \frac{3982155}{10000000} \times a$; and putting,

inſtead of a, $\frac{a\pi}{\pi} = \frac{a\pi}{\frac{22}{7}} = \frac{7}{22}\, a\pi = \frac{7}{22} \times 180$ deg.

(ſince $a\pi$ is the ſemi-circumference of the circle wherein this inclination is performed) we ſhall have $\frac{3}{365\frac{1}{4}} \times \frac{1}{4 \times 176} \times \frac{3982155}{10000000} \times \frac{7}{22} \times 180$ deg. = (after reduction) $57'''.25''''$.

Problem XV.

39. *To find the difference of the inclination of the earth's axe to the plane of the lunar orbit, when the moon arrives at its point of ſtation, 90 degrees diſtant from the node of the equator with the lunar orbit, this difference of inclination being cauſed by the moon.*

By *Art.* 18. the variation of inclination will be $\frac{1}{2} B \times \frac{M}{2\pi r} \times a\, a$.

Putting, inſtead of B, its value of *Art.* 30.; inſtead of M, which expreſſes the time of the moon's revolution about the earth, $27\frac{1}{3}$ days; inſtead of $\frac{a}{r}$, the mean value $\frac{3982155}{10000000}$; I ſay *mean value*, becauſe the earth's axe is not always equally inclined to the plane of the lunar orbit, as has been ſhewn in *Art.* 19. *&c.* we ſhall have $\frac{1}{2} \times \frac{3 \times \frac{5}{2}}{(365\frac{1}{4})^2} \times \frac{27\frac{1}{3}}{2 \times 176} \times \frac{3982155}{10000000} \times a$ = (putting, as in the preceding

ceding *Problem*, instead of a, $\frac{7}{22} \times 180$ degrees) $\frac{1}{2} \times \frac{3\frac{5}{2}}{(365\frac{1}{4})^2} \times \frac{27\frac{1}{3}}{2 \times 176} \times \frac{3982155}{10000000} \times \frac{7}{22} \times 180$ degrees, = (after reduction) $10''', 19''''$.

Problem XVI.

40. *To find the motion of the node of* Jupiter's *fourth ſatellite, cauſed by the ſun, during the time of* $\frac{1}{4}$ *of a revolution of that ſatellite about the ſun.*

We need only make uſe of the *formula* of *Problem* XI. wherein, conformably to *Art.* 25. we muſt make $\frac{a}{1-2a} = 1$, $t = 16\frac{3}{4}$ days, being the time of the revolution of the ſatellite about *Jupiter*; $T = 365\frac{1}{4} \times 12$ days, which is the time of the ſatellite's revolution about the ſun, or about 12 years; $\frac{b}{r}, = 1$, becauſe the orbit of the ſatellite is but little inclined to the ecliptic, and, conſequently, the axe thereof nearly at right angles thereto.

We ſhall have then $\frac{3t}{T} \times \frac{\pi g}{8}$, or $\frac{3 \times 16\frac{1}{4}}{365\frac{1}{4} \times 12} \times \frac{180 \text{ deg.}}{8}$.

This is the motion of the node of this ſatellite in 3 years, or during $\frac{1}{4}$ of its revolution about the ſun.

Remark.

41. By this may be found the motion of the nodes of the reſt of *Jupiter*'s ſatellites. For it appears, from the *formula*, that the motion of the nodes of different

different ſatellites of the ſame planet, is as the times of their revolutions about the planet.

Thus the motion of the nodes of the 3d ſatellite, in the ſpace of 3 years, is $\frac{3 \times 7\frac{1}{6}}{365\frac{1}{4} \times 12} \times \frac{180 \text{ deg.}}{8}$.

The motion of the nodes of the 2d is $\frac{3 \times 3\frac{1}{24}}{365\frac{1}{4} \times 12} \times \frac{180 \text{ deg.}}{8}$.

The motion of the nodes of the 1ſt is $\frac{3 \times 1\frac{1}{4}}{365\frac{1}{4} \times 12} \times \frac{180 \text{ deg.}}{8}$.

PROBLEM XVII.

42. *To find the motion of the nodes of the 5th ſatellite of* Saturn, *cauſed by the ſun, during the time of* $\frac{1}{4}$ *of the revolution of this ſatellite about the ſun.*

Conformably to the preceding *Problem* we ſhall have, by putting inſtead of t, $79\frac{1}{3}$ days; inſtead of T $365\frac{1}{4} \times 30$ days, which is nearly the time of the revolution of this ſatellite about the ſun, and we ſhall have $\frac{3 \times 79\frac{1}{3}}{365\frac{1}{4} \times 30} \times \frac{180 \text{ deg.}}{8}$.

43. In the ſame manner may be found the motion of the nodes of all the reſt of *Saturn*'s ſatellites.

PRO-

Problem XVIII.

44. *To find the motion of the moon's node during the time that the ſun is paſſing from the node of the moon's orbit with the ecliptic, to its point of ſtation; that is, in three months, wanting a few days.*

Here we are to make uſe of the *formula* of *Art.* 13. becauſe the motion of the moon's nodes is very ſenſible in reſpect of the motion of the ſun.

We ſhall have $Ab \times \frac{T}{2r} \times \frac{g}{4} \times (1 - Ab \times \frac{T}{2\pi r} \times \frac{3}{4})$; and employing the values of *Prob.* XI. except that here $t = 27\frac{1}{3}$ days, which is the time of the moon's revolution about the earth, and that $\frac{b}{r} =$ to the ſine of 85 deg. divided by the radius; the axe of the lunar orbit being inclined in an angle of about 85 degrees to the ecliptic. Therefore $\frac{b}{r} = \frac{9961947}{10000000}$. Laſtly, by *Art.* 25. we muſt take $\frac{a}{1-2a} = 1$, and the preceding *formula* will be changed into this, $\frac{3 \times 27\frac{1}{3}}{365\frac{1}{4}} \times \frac{9661947}{10000000} \times \frac{180 \text{deg.}}{8} \times (1 - \frac{3 \times 27\frac{1}{3}}{365\frac{1}{4}} \times \frac{9961947}{10000000} \times \frac{3}{2 \times 8}) =$ (after reduction) $4^{\circ}. 36'. 33''$.

This is the motion of the moon's node during the time that the ſun is paſſing from the node of the moon's orbit with the ecliptic to its point of ſtation; that is, during the time the ſun is moving 90 — 4 degrees

grees, 36 minutes, 33 ſeconds, or 85°. 23′. 27″. And if the motion of the node be required for one year, 'tis only multiplying the motion, juſt found, by $\frac{360^\circ}{85^\circ.23'.27''.}$, and it will be found 19°. 25′. 39″. being a very ſmall matter more than the truth, for reaſons which will be explained elſewhere, and becauſe we have taken $27\frac{1}{3}$ days for the time of the moon's revolution about the earth, which is really a little leſs than that quantity.

Problem XIX.

45. *To find the greateſt difference of the inclination of the lunar orbit to the ecliptic; which happens when the ſun arrives at his point of ſtation with regard to the moon.*

Here the *formula* of *Art.* 15. muſt be made uſe of, which gives $\frac{1}{2}Aaa \times \frac{T}{2\pi r} \times \left(1 - \frac{1}{2}Ab \times \frac{T}{2\pi r}\right)$, or, which is the ſame, $\frac{1}{2}A \times \frac{aT}{2\pi r} \times \frac{a\pi}{\frac{22}{7}} \times \left(1 - \frac{1}{2}A \times \frac{bT}{2\pi r}\right)$, ſubſtituting the values as in the preceding *Problem*, and inſtead of $\frac{a}{r}$, putting $\frac{871557}{10000000}$, which is the ratio of the ſine of 5° to radius, we ſhall have $\frac{3 \times 27\frac{1}{3}}{365\frac{1}{4}} \times \frac{1}{2 \times 2} \times \frac{871557}{10000000} \times \frac{7}{22} \times 180 \text{ deg.} \times \left(1 - \frac{1}{4} \times \frac{3 \times 27\frac{1}{3}}{365\frac{1}{4}} \times \frac{9961947}{10000000}\right) = 15'.51''.$

Remark.

Remark.

46. It ſhould be obſerved, that, to ſhorten the computations, I have contented myſelf with taking the times of the revolutions pretty near the truth; but if the utmoſt exactneſs be required, the accurate times of the revolutions muſt be employed.

47. This might be a proper place to add the method of determining the perturbation of the orbit of any planet, as derived from another planet; but ſince this depends upon no other than the very ſame principles that have been made uſe of in this memoir, and as their application will be ſhewn, in its full extent, in the memoir which I am going to print, and intend myſelf the honour of ſending to the *Royal Society*, I ſhall deſiſt, that I may not run this paper to a greater length.

LIX. *A Letter to the Right Honourable* George *Earl of* Macclesfield, *P. R. S. concerning the ages of* Homer *and* Heſiod. *By* George Coſtard, *M. A.*

My Lord,

Read Dec. 13, 1753. IT ſeems to be an opinion pretty generally received, that Homer and Heſiod lived much about the ſame time. If this be true, and they did ſo, whatever arguments prove the age of one, will equally ſerve for fixing that of the other. What that age was, is indeed not at all agreed on among writers; the only thing in which they con-

 ſpire

ſpire, being, I apprehend, to place both of them much earlier than they ought to have done.

Among the ancients, Velleius Paterculus (1), as now printed at leaſt, ſays, that Homer lived 950 years before his time. This author dedicates his hiſtory to the conſul Vinicius, who is placed in the *faſti conſulares* A. V. C. 782. which is A D. 30. So that, according to this computation, Homer muſt have flouriſhed about the year before Chriſt 920. And with this account agrees pretty nearly the Parian marble (2).

Herodotus, according to our preſent copies of him, placeth Heſiod and Homer *not more* than 400 years before his time (3). Herodotus, according to A. Gellius (4), was 53 years old at the beginning of the Peloponneſian war, or the year before Chriſt 431. And

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(1) Hic longius a temporibus belli, quod compoſuit, Troici, quam quidam rentur, abfuit. Nam ferme ante annos DCCCCL. floruit, intra mille natus eſt. *Hiſt. Rom. l.* i. *c.* 5. In the *Faſt. conſular.* as publiſhed by Cardinal Noris, the conſuls Vinicius and Longinus are placed the year following, or A. V. C. 783.

(2) ΑΦΟΥ ΟΜΗΡΟΣ Ο ΠΟΙΗΤΗΣ ΕΦΑΝΗ ΕΤΗ[Η] ΗΔΔΔΔΙΗ ΒΑΣΙΛΕΥΟΝΤΟΣ ΑΘΗΝΩ .... ΙΟΓΝΗΤΟΥ. N° 45. where ſee the commentators.

(3) Ησιοδον γὰρ καὶ Ομηρον ἡλικίην τετρακοσίοισι ἔτεσι δοκέω μεν πρεσβυτέρους γενέσθαι, καὶ ὐ πλέοσι. Pag. 109. *Edit. Gronov.*

(4) *Noct. Attic. l.* xv. *c.* 23. And if Xerxes came into Greece in the year before Chriſt 480. as is commonly ſuppoſed, then Homer muſt have lived, according to Herodotus, at moſt, but about 400 years before that expedition, But in this Herodotus differs widely from himſelf, if he is the author of the *Life of Homer* commonly attributed to him. For there he ſays, ἀφ' ἧς ὴ Ομηρῳ ἐγένετο, ἔτεά ἐστιν ἑξακόσια εἰκοσιδύο μέχρι τῆς Ξέρξεω διαβάσεως, *ſub fin.* See Bayer in the *Act. Petropol.* vol. 3. p. 338. where he rejects this piece as ſpurious.

if

if to this we add 400 years, we ſhall have the year before Chriſt 831. about which time conſequently, according to him, both Homer and Heſiod muſt have flouriſhed.

Among the moderns, Petavius (5) places Heſiod A. P. J. 3714. or about the year before Chriſt 1000. and in his *Rationarium Temporum* (6) he ſays, that Heſiod was contemporary with him, and that this *ex* ARCTURI ORTU, *quem poeta iſte deſcribit, eruditi artis illius colligunt*; and in the margin refers to Longomontanus in his *Aſtronomia Danica* (7).

With Petavius agrees very nearly Palmerius, as cited by Dr. Hyde in his notes on Ulug Beigh (8), tho' Sir Iſaac Newton (9), whoſe authority with ſome perſons is deciſive, tells us, that from the achronical riſing of the ſame ſtar it follows, that Heſiod flouriſhed about 100 years after the death of Solomon. This again he places, in his ſhort chronicle, in the year before Chriſt 979. from which, if we ſubtract

(5) Uranolog. l. vii. c. 5.

(6) Part I. l. i. c. 12.

(7) And in this he hath the authority of Aulus Gellius, l. xvii. c. 21. who ſays, 'De Homero & Heſiodo inter omnes fere ſcriptores conſtitit, ætatem eos egiſſe vel iiſdem ferè temporibus, vel Homerum aliquanto antiquiorem; utrumque tamen ante Romam conditam vixiſſe, Silviis Albæ regnantibus, annis poſt bellum Trojanum, ut Caſſius in primo annalium de Homero atque Heſiodo ſcriptum reliquit, plus centum atque ſexaginta, ante Romam autem conditam, ut Cornelius Nepos in primo chronicorum de Homero dixit, annis circiter centum & ſexaginta.' The building of Rome is commonly placed the year before Chriſt 752. To this add 160 years, and Homer and Heſiod will both, according to Cornelius Nepos, have lived about the year before Chriſt 912.

(8) Page 3.

(9) *Chronology*, p. 95.

100 years, we ſhall have the year before Chriſt 879. when, according to him, both Heſiod and Homer, if contemporaries, muſt have flouriſhed. In what manner Sir Iſaac Newton computed this, or whether indeed he ever computed it at all himſelf, is not, at leaſt publickly, known. It is probable he only followed ſome one elſe; and therefore, without derogating in the leaſt from his authority, or thinking it a failure in reſpect to the memory of the greateſt man that ever lived, I ſhall conſider a little how far the age of theſe poets may be determined, with any certainty, from this achronical riſing of Arcturus.

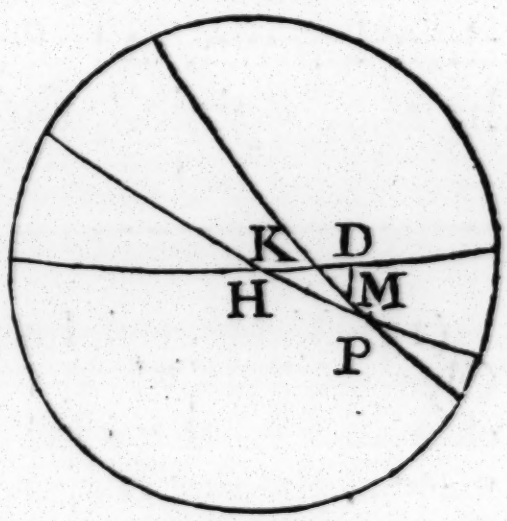
Longomontanus, in his *Aſtron. Danic.* (10) ſuppoſeth Heſiod to have flouriſhed about the year before Chriſt 776. when he makes the place of Arcturus ♍ 12° 16′, the place of the Sun's apogee ♉ 20° 10′, and his place, 60 days after the winter ſolſtice, ♓ 1° 10′. In the year after Chriſt 1610. he ſays, the place of Arcturus was ♎ 18° 47′; ſo that from the year before Chriſt 776. to the year 1610. Arcturus had moved through 36° 31′, = 131460″; which divided by 2386, the number of years elapſed, gives the annual motion of the fixed ſtars 55″. But as he makes the annual motion of the fixed ſtars 49″ 45‴, or 1° in 72⅓ years; 55″ will, according to him, require about 2658 years. So that Heſiod, according to his computation, muſt have lived about the year before Chriſt 1048; unleſs, as he ſeems to ſuſpect, that poet deſcribes the riſing of Arcturus, not

(10) Lib. II. *Sphæric.* cap. iv. prob. 2.

as it was in his own time, but 272 years before. So that from hence, we ſee, nothing certain can be concluded with regard to his age.

Kepler, in his *Epitom. Aſtronom.* (11) ſuppoſeth, that from the time of Heſiod to the year after Chriſt 1618. are 2400 years, and that the annual motion of the fixed ſtars is 51″, which, in 2400 years, gives 34°. From whence, and ſeveral other aſſumptions, he concludes, that, in Heſiod's time, Arcturus roſe achronically March 3. in the Julian year reckoned backward, when the Sun was in ♓ 5° 11′.

Riccioli, in his *Almageſt.* (12) ſuppoſeth, that Heſiod flouriſhed about the year before Chriſt 775. when the place of the Sun's apogee was ♉ 20°; and therefore the Sun's true motion for 60 days was 61° 10′, which added to the place of the winter ſolſtice, or the beginning of ♑, gives the Sun's place ♓ 1° 10′, the point oppoſite to that point of the ecliptic which roſe along with Arcturus, or ♍ 1° 10′. Therefore, in the figure here annexed, according to him, the point K is ♍ 1° 10′, and K P the diſtance from the next equinoctial point, = 28° 50′. The height of the equator at Athens, or the angle P H K, from Ptolemy's *Geography*, = 52° 15′. He farther ſuppoſes, as Longomontanus before him, the ob-



(11) Lib. III. p. 396.
(12) Tom. I. p. 463.

liquity

liquity of the ecliptic, or the angle, H P K, = 23° 32′; from whence he finds the angle P K H = 107° 43′, and the complement of it M K D = 72° 17′. He aſſumes likewiſe the latitude of Arcturus, or M D, = 31° 3′ north; from whence he finds the arc K M = 11° 5′; which added to the point K, or ♍ 1° 10′, gives the place of Arcturus, or M, = ♍ 12° 15′.

But at the end of the year 1644. the place of Arcturus, he ſays, was ♎ 18° 19′; therefore from the time of Heſiod, before aſſumed, to the end of the year 1644. that ſtar had moved through 36° 4′. But this it would do, he ſays, in 2597 years. From whence, therefore, ſubtracting 1644, there remains the year before Chriſt 953. He concludes, therefore, as Longomontanus, we ſaw, ſuſpected before, that Heſiod ſpeaks of the achronical riſing of this ſtar, not as it was in his own time, but two centuries before. Beſides, as the refraction of Arcturus would accelerate his riſing, and the Sun's refraction would retard his ſetting; and as the time of the ſolſtice was then known, at beſt, but in a very groſs manner (13); he is of opinion, that this method is not much to be

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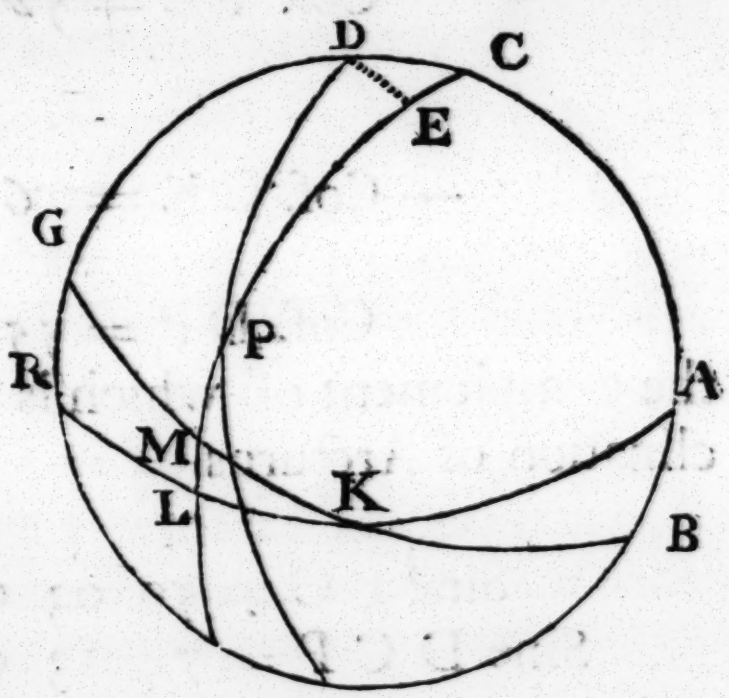
(13) Meton and Euctemon obſerved the time of the ſolſtice Olymp. LXXXVI. 4. or the year before Chriſt 432. and Ariſtarchus Samius afterwards; but Ptolemy ſays they were very rudely made: And that Hipparchus, before him, was of the ſame opinion. Ενεκεν ὧν τῆ καθόλε τὲ τὰς τῶν τροπῶν τηρήσεις δυσδιακρίτως ἔναι, κὴ πρὸς τόυτοις τὰς ὑπ' ἐκείνων παραδεδομένας ὁλοχερέστερον εἰλημμένας, ὡς κὴ τῷ Ἱππάρχῳ δοκεῖ φαίνεσθαι, τάυτας μὲν παρητησάμεθα. *Syntax.* p. 62. But if this was the caſe of obſervations then made, what muſt we ſuppoſe it to have been two or three hundred years before their time?

depended

depended on; contrary to what Scaliger (14) and Voſſius (15) both thought.

As there are, however, ſeveral errors in this computation, it may not be amiſs, perhaps, to form another, upon ſuppoſition, with Sir Iſaac Newton, that Heſiod flouriſhed about the year before Chriſt 879. or, in round numbers, the year 880. and let us ſee what will be the reſult of it.

At the end of the year 1689. the place of Arcturus, in the Britiſh catalogue, was ♎ 19° 53′ 52″, or 6ˢ 19° 53′ 52″; and from the year before Chriſt 880. to the end of the year 1689. are 2569 years, the preceſſion for which time is 1ˢ 5° 40′ 50″: This, ſubtracted from the place of Arcturus 6ˢ 19° 53′ 52″, gives his place, in the year before Chriſt 880, = 5ˢ 14° 13′ 02″. The latitude of this ſtar, is, in the ſame catalogue, = 30° 57′. Therefore, in the figure here, we have G M K B the ecliptic, R L K A the equator, C P the complement of the ſtar's



(14) Heſiodus florebat eo ſæculo quo Arcturus ἀκρόνυχος oriebatur in Bœotia VIII die Martii, ſi quid hoc ad conjecturam facit, ſaltem apud excellentes aſtrologos, qui ex hoc parapegmate, infra Septuaginta plus minus annos, ſæculum Heſiodi deprehendere poſſunt. *Animadverſ. ad Euſeb. Chron. Num.* MCCLV.

(15) Operæ vero eſt attendere ad id quod Heſiodus ipſe ſcribat, ſua ætate Arcturum ἀκρόνυχον in Bœotia exortum fuiſſe VIII die Martii: Unde poetæ hujus ætas in tantum ſaltem poſſit colligi, ut error ſi quis ſit ſaltem intra LXX annos ſit conſtitutus. *Voſſ. de Poet. Græc. L.* i. *c.* 2.

latitude,

latitude, = 59° 3′, DC the diſtance of the poles of the ecliptic and equator, = 23° 29′, and the angle DCE, whoſe meaſure is GM, the ſtar's longitude from the next ſolſticial colure, = 74° 13′ 02″.

Then rad. + cof. DCP = 74° 13′ 02″ — 19.4345545
— Cotang. DC = 23 29 00 — 10.3620437

Tang. CE = 6 44 25 — 9.0725108
PC = 59 03 00

PE = 52 18 35
Then cof. DC = 23 29 00 — 9.9624527
Cof. PE = 52 18 35 — 9.7863203

19.7487730
— Cof. CE = 6 44 25 — 9.9969879

Cof. DP = 55 37 20 — 9.7517851

the complement of which is = 34° 22′ 40″ = the declination of Arcturus.

Sine PC = 59° 03′ 00″ — 9.9332931
Sine DCP = 74 13 02 — 9.9833104

19.9166035
Sine DP = 55 37 20 — 9.9166290

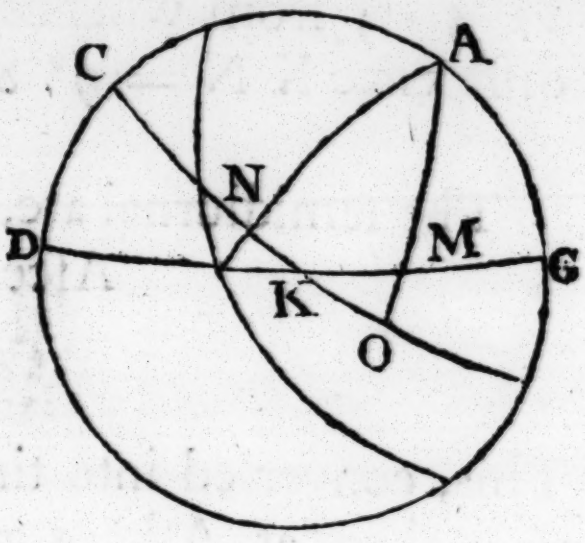
Sine PDC = 89 22 50 — 9.9999745 the complement of which is PDG = 90° 37′ 10″. The right aſcenſion, therefore, of Arcturus, at that time, was = 180° 37′ 10″.

Where

Where this obſervation on Arcturus was made, is not ſaid; we may ſuppoſe it to have been at Aſcra, where Heſiod's father lived, as he tells us himſelf (16). But as the ſituation of this place is not very well known (17), we may, without any ſenſible error, take Athens, whoſe latitude is made, by the beſt modern geographers, 38° 5′ north.

In the figure, then, here, we have M O = the declination of Arcturus, as before, = 34° 22′ 40″.

A G = the height of the pole at Athens, = 38° 5′, the complement of which, M K O, = D K C = 51° 55′. Therefore



Tangent M O =	34	22	40	—9.8352480
— R. + cot. M K O =	51	55	00	—9.8941114
Sine K O =	32	25	10	—9.7293594

the aſcenſional difference; which ſubtracted from the right aſcenſion before found, gives the oblique aſcenſion = 148° 12′.

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(16) Νάσσατο δ' ἀΐσχ' Ελικῶνος ὀϊζυρῇ ἔνι κώμῃ
ΑΣΚΡΗ χείμα κακῇ, θέρει ἀργαλέῃ, οὐδέποτ' ἐσθλῇ.
Op. & Dieſ. l. ii. *v.* 257.

(17) Κεῖται μὲν ὧν (inquit Proclus) ὑπὲρ τὴν ὁδὸν ἣν βαδίζουσιν οἱ ἐπὶ τὸ Μουσεῖον ἀπίοντες, αὐτῆ ἡ Ασκρη τοῦ ὄρους Ελικῶνος ἐκκειμένη τοῖς ἀνέμοις, καὶ θαυμασὰς μὲν ἀναπαύλας ἔχουσα ἐν θέρει, δυσνέμου δ' οὔσης ἐν χειμῶνι, τὴν Ασκρην ἐν τῷ Μεσημβρινῷ κειμένην τοῦ ὄρους, τὸ μὲν ἐπὶ τῶν ἀνέμων ἀπολαύειν βίας, *in loc.*

In the year before Chriſt 880. the time of the winter ſolſtice was December 29. at 15 minutes paſt ſix o'clock in the morning, according to the vulgar reckoning; or, in the aſtronomical account, $28^d\ 18^h\ 15'$; and 60 days after this, brings us to February 27. when the Sun's place was $11^s\ 00°\ 6'\ 23''$; his declination ſouth $11°\ 27'\ 18''$; his right aſcenſion $332°\ 11'\ 56''$; from whence we ſhall have his aſcenſional difference K N = $9°\ 8'\ 15''$. Then

	°	′	″
The ſemidiurnal arc, in a right ſphere, is	90	00	00
Aſcenſional difference	9	08	15
Semidiurnal arc	80	51	45

This, converted into time, gives the time of Sun-ſetting then at Athens $5^h\ 23'\ 27''$; from whence we ſhall have the nocturnal arc $13^h\ 13'\ 6''$.

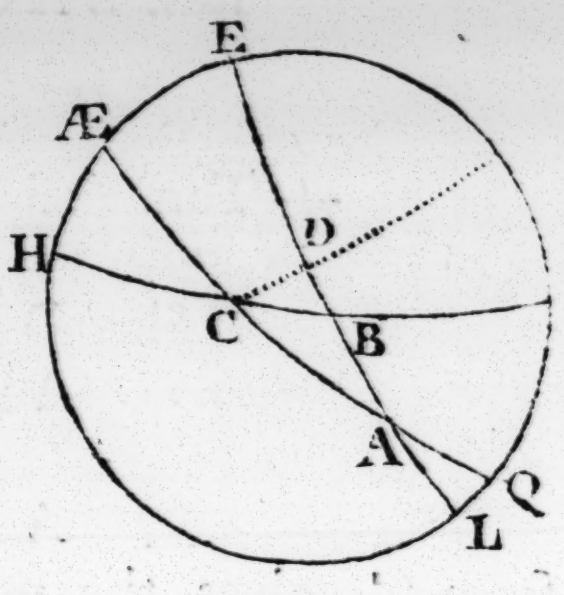
	°	′	″
Again; the Sun's oblique aſcenſion is	341	19	11
Oblique aſcenſion of Arcturus	147	52	40
Difference	193	26	31

	h	′	″
This, converted into time, gives	12	53	46
Nocturnal arc	13	13	06
Difference	00	19	20
Semidiurnal arc, add	5	23	27
Time of Arcturus's riſing	5	42	47

By this it appears, that at Athens, in the year before Chriſt 880, and 60 days after the winter tropic, the ſtar Arcturus roſe at $19'\ 20''$ after Sun-ſetting.

But

But if we would inquire the time when it roſe achronically, in the proper ſenſe of the word, we have, in the figure here, E A L the ecliptic, Æ A Q the equator, C D a portion of a ſecondary of the ecliptic perpendicular to E A L, A C the diſtance of the point of oblique aſcenſion from the autumnal interſection = 31° 48′; we have likewiſe the angle C A B the obliquity of the ecliptic, = 23° 29′, and the angle Æ C H the height of the equator at Athens, = 51° 55′ = A C B.



		°	′	″	
Then	Tan. B A C	23	29	00 —	9.6379563
— Rad. +	cof. A C	31	48	00 —	9.9293641
	Cot. A C D	69	44	00 —	9.5673204
—	A C B	51	55	00	
	Sine D C B	17	49	00 —	9.4856820
	Cof. B A C	23	29	00 —	9.9624527
				Sum	19.4481347
—	Sine A C D	69	44	00 —	9.9722448
	Cof. A B C	107	24	25 —	9.4758899
Then	Cof. B A C	23	29	00 —	9.9624527
— Rad. +	Tan. A C	31	48	00 —	9.7924101
	Tan. A D	29	37	35 —	9.7548628

Sine AD	29	37	35	9.6940277
Tan. BAC	23	29	00	9.6379563
			Sum	19.3319840
— Tan. ABC	107	24	25	10.5037429
Sine DB	3	51	38	8.8282411
AD	29	37	35	

AB 25 45 57 which taken from 6°, gives the point of the ecliptic rising with Arcturus; *i. e.* ♍ 4° 14′ 3″; the point opposite to which is ♓ 4° 14′ 3″. Then

Longit. of the Sun from the equinox	11	04	14	03
Precession of the equinox, subtract	11	24	32	00
Longit. of the Sun from 1st * of ♈	11	09	42	03
Mean anomaly corresponding	8	29	22	59
Subtract 880	6	28	27	12
	2	00	55	47
March	1	28	09	04
	2	46	43	
2d	1	58	16	
			48	27
19h			46	49

So that the Sun entered ♓ 4° 14′ 3″, that year, March 2. at 19h, in the astronomical account; or, in the vulgar

ulgar way of reckoning, at 7 o'clock in the morning, March 3. Either this day, therefore, or the preceding one, might, at that time eſpecially, have been taken, indifferently, for the day when Arcturus roſe achronically.

But tho' this is what is properly meant by achronical riſing (19); yet as a ſtar, at that time, is inviſible, and, conſequently, can be no rule for huſbandmen, for whoſe uſe theſe obſervations were intended; there is another achronical riſing, called the apparent one: This is when a ſtar firſt appears above the eaſtern horizon after ſun-ſet (20); which, therefore, requires ſome certain depreſſion of the Sun in the oppoſite part of the heavens, more, or leſs, according to the magnitude of the ſtar required to become viſible.

It was ſaid before, that in the year before Chriſt 880. Feb. 27. Arcturus roſe, at Athens, 19′ 20″ after Sun-ſet; but whether this, tho' a bright ſtar of the firſt magnitude, could be ſeen there ſo ſoon in the eaſtern horizon as even at 30 min. paſt Sun-ſet, may well be queſtioned: And therefore Feb. 27. or the 60th day after the winter ſolſtice, could not be there eſteemed the day of the apparent achronical riſing of Arcturus.

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(18) Ευτ ἂν δ' ἑξήκοντα μετὰ τροπὰς ἠελίοιο
Χειμέῃ ἐντελέσῃ Ζεὺς ἤματα, δὴ τότ' Ἀστὴρ
Ἀρκτοῦρος, προλιπὼν ἱερὸν ῥόον ὠκεανοῖο
Πρῶτον παμφαίνων, ἐπιτέλλεται ἀκρονύκτιος.

Op. & Dieſ. l. ii. *v.* 185.

(19) Εσπερία δ' ἐστὶν Ἐπιτολὴ, ὅταν τῷ ἡλίῳ δυνομένῳ, ἐπιτέλλῃ τὶς Ἀστὴρ ἅμα καὶ τῇ ἀρίζοντι γενόμενος. *Gemin. apud Petav. Uranolog. cap.* xi.

(20) Οταν ὁ μετὰ τὴν τοῦ ἡλίου δύσιν πρῶτος ἐπισφυγὼς τὰς αὐγὰς τοῦ ἡλίου θεωρηθῇ, τότε λέγεται φαινομένην ἑσπερίαν ἐπιτολὴν ποιεῖσθαι. ἐν ᾗ ταῖς ἐχομέναις νυξὶ μεγεθρότερος ἀεὶ μᾶλλον, καὶ μᾶλλον φαίνεται. *Gemin. ibid.*

I have

I have hitherto called it the ſtar Arcturus; but it is not improbable that Heſiod meant the whole conſtellation Bootes (21). He calls it, indeed, ΑΣΤΗΡ; and that word, according to Macrobius (22), ſignifies only a *ſingle ſtar*. But whatever it might do in his time, it ſeems evident, that, among the antients, and eſpecially the poets, that diſtinction was not always nicely obſerved (23). If this, therefore, ſhould be the caſe with reſpect to Heſiod, the time of this riſing of Arcturus will be ſomething more indeterminate, as a conſtellation cannot riſe all at once, nor is it now known how many ſtars this conſtellation, in particular, was, in thoſe early times, ſuppoſed to conſiſt of.

But farther; it hath been hitherto taken for granted, that Heſiod is to be underſtood as ſpeaking of Aſcra, or ſome place in the neighbourhood of it; but this, likewiſe, is not altogether certain: For it was no unuſual thing with the antients to ſet down in calendars, of this ſort, obſervations on the riſings and

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(21) Ἀρκτῦρος ὃ λέγεται καὶ αὐτὸς ὅλος ὁ Βοώτης, ἰδίως δὲ καὶ ὁ ὑπὸ τὴν ζώνην ἀστὴρ ἀπὸ ἑνός. *Suid. in voc.* Ἀρκτος. And ſo Theon; Ενα δ' ἔχει (Arctophylax) ἐν μέσῃ ζώνῃ ὃς διὰ τὴν ὑπερβολὴν τοῦ λαμπρότητος ἰδίως καὶ αὐτὸς λέγεται Ἀρκτουρος, ὁμοίως τῷ παντὶ Ἀρκτῴρῳ. In *Arat. Phænom.* p. 15.

(22) Sic. & apud Græcos Aſter & Aſtron diverſa ſignificant, & Aſter ſtella una eſt; Aſtron ſignum ſtellis coactum, quod nos ſidus vocamus. *In Som. Scip. l.* i. *c.* 14. Ἀστὴρ ὃ Ἀστρα διαφέρει· ὅτι ὁ μὲν Ἀστὴρ ἕν τι ἐστί· τὸ δὲ Ἄστρον ἐκ πολλῶν συνέστηκεν Ἀστέρων, Ζῴδιον ὄν· ὃ καὶ Ἀστροθέτημα καλεῖται. *Didym. in Illiad.* iv. *v.* 75.

(23) Aratus uſeth the words Ἀστὴρ and Ἄστρον indifferently.

——Σήματ' ἐν οὐρανῷ ἐστήριξεν
Ἄστρα διακρινας ἐσχέψατο δ' εἰς ἐνιαυτὸν
Ἀστέρας, οἵ κε μάλιστα τετυγμένα σημαίνοιεν
Ανδράσιν ὡραίων, ὅτε τ' ἔμπεδα πάντα φύωνται

Νῦν δὲ ὁ Αρατος, ſays the Scholiaſt there, τὸς Ἀστέρας Ἄστρα εἴρηκε. *Pag.* 3. *Ed. Oxon.*

ſettings

ſettings of the ſtars made in very diſtant times and countries (24); the latitudes of places being unattended to, and the ſlow motion of the fixed ſtars about the poles of the ecliptic unknown, and indeed unſuſpected, or diſregarded afterwards, when it became ſuſpected.

But tho' we ſhould grant the place of obſervation to have been at, or near Aſcra, yet there will ſtill remain a difficulty, with reſpect to the time. In the computation before given it hath been ſuppoſed, that Arcturus roſe there achronically on the 60th day from the ſolſtice, excluſive of the ſolſticial day itſelf; but as the particle μετὰ is ſometimes taken inclu-ſively (25), we may reckon the day of the ſolſtice it-

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(24) See the calendars in *Petav. Uranolog.* In qua aliorum & temporum & climatum confuſione, præ aliis maxime hallucinatus eſt Manilius; qui Ægyptiaci cœli deſcriptiones Romano adaptaſſe, & Græcanicæ Barbaricæque ſphæræ obſervationes, nullo judicio, ſimul commiſcuiſſe deprehenditur. *Bainbrig. Canicular. p.* 22. See, likewiſe, *Dodwel. Append. ad Cyprian. Diſſert. p.* 19.

(25) Thus μετ' ἡμέρας δύο duobus diebus poſt. *Demoſth. in Mid.* and μετὰ τρίτον ἔτος in *Diod. Sic.* p. 103. *Edit, Wechel.* Poſt ſeptem luces. *Ovid. Faſt.* l. vi. v. 774. and what is τῇ τρίτῃ ἡμέρᾳ ἀναστῆναι, *Luke* xxiv. 7. is μετὰ τρεῖς ἡμέρας ἐγείρομαι, *Matt.* xxvii. 63. and μετὰ πεντήκοντα καὶ ἑκατὸν ἡμέρας, in the LXX. *Gen.* viii. 3. is on the 150th day, as appears from the next verſe. The *Arabs* uſe their particle بعد likewiſe in the ſame manner. Thus حولت القبلة الي الكعبة في رجب بعد الهجرة سبعة عشر شهرا وقيل في شعبان .....وحرمت الخمر بعد الهجرة باربع سنين في السنة الرابع converſa eſt Keblah ad Caabam menſe Rageb, menſibus poſt fugam 17. vel juxta alios menſe Shaaban ..... interdictum vero vinum 4 poſt fugam annis, anno ſc. quarto. *Alkodaius in Pocock. Spec. Hiſt. Arab. p.* 175.

ſelf

ſelf one of the number, which, conſequently, will bring us only to Feb. 26.

Beſides; what hath been ſaid, hath been built upon the ſuppoſition that the day of the ſolſtice was, at that time, preciſely known; a thing, however, not haſtily to be granted. The inaccuracy of obſervations, and the want of proper inſtruments, in times much later than this we are here ſpeaking of, would incline one not to attribute too much to them, in a caſe of ſo much nicety. Since, then, we find the ſolſtice fell out ſo early in the morning; either December the 28th, or 29th, might have been taken for the ſolſticial day: And, accordingly, 60 days after will be either February the 26th, or 27th. But as the Sun's change of declination, at that ſeaſon of the year, is very ſlow (26); an error of a day, or two, or more, either forward, or backward (a thing by no means impoſſible), will bring us to Feb. 25. or 28. which is a difference of no leſs than 4 days.

If any one thinks ſuch a miſtake as this incredible, let it be obſerved, that in the calendar prefixed, in ſome editions, to Ovid's *Faſti*, the Sun is ſaid to enter Aquarius XV kalend. Feb. or Jan. 18. Ovid himſelf ſeems to place it XVI kalend. Feb. or Jan. 17. and with him agrees Pliny; tho' Columella, under the reign of Claudius, and Ptolemy, under Antoninus Pius, place it one day earlier, or the XVII kalend. Feb. Here is plainly a difference of 3 days, and yet all of them wrong: For Ovid, as is generally agreed, inſcribed his *Faſti* to Germanicus ſoon after

(26) Keill. *Lect. Aſtron. p.* 250.

his

his baniſhment, or about *A. D.* 10. but, by the tables, the Sun entered Aquarius, that year, Jan. 21. and in the ſecond year of Antoninus Pius, or *A. D.* 139. when Ptolemy obſerved the fixed ſtars, he entered the ſame ſign Jan. 20^{d} 16^{h}, or at 4 o'clock in the morning Jan. 21. according to the vulgar reckoning.

But if ſuch miſtakes could be committed at this time, how little muſt we ſuppoſe the true time of the ſolſtice known, ſo early as the year before Chriſt 880.

But not to aſſume too much, let us ſuppoſe a miſtake of two days only, in the riſing of Arcturus. By calculating as before, we ſhall find, that *A. C.* 1689. the point of the ecliptic riſing along with Arcturus, in the latitude of Athens, was ♎ 10° 35′ 55″, the point oppoſite to which is ♈ 10° 35′ 55″. But this point the Sun entered, that year, March 20. when, conſequently, Arcturus roſe there achronically: But in the year before Chriſt 880. as before obſerved, Arcturus might be ſaid to riſe achronically there March 2. this gives a difference of 18 days in 2569 years; from whence a difference of 2 days will give 285 years, which ſubtracted from the year before Chriſt 880. will give the year before Chriſt 595. for the time of Heſiod, and, conſequently, of Homer too, if contemporary with him, for any thing that can be gathered to the contrary from the achronical riſing of Arcturus.

Having now ſhewn, in this manner, what little preciſion there is in this argument, I might, as I at firſt intended, take my leave of the ſubject, and refer the ſettling the age of theſe two poets to authorities of

another nature. But as the favourers of their high antiquity will, I queſtion not, be ſtartled to hear that their age may be brought down ſo low as the year before Chriſt 595. your lordſhip will not be diſpleaſed, I hope, if I add ſomething farther in confirmation of this date, and ſhew, that it is not ſo unreaſonable, or abſurd, to fix them at this very time, as at firſt ſight it may appear.

I ſhall not trouble your lordſhip with a variety of philological arguments, that, I think, I could produce in ſupport of this aſſertion. That would ſwell this letter beyond the bounds of your lordſhip's patience: I ſhall therefore confine myſelf to a few internal evidences alone, taken from the poets themſelves; which, being of an aſtronomical nature, will, I flatter myſelf, on that account, at leaſt, recommend themſelves to your lordſhip's attention.

The firſt that ſhall be offered, ſhall be from the following lines of the *Iliad* itſelf.

Οἷον δ' ἀςέρα πέμπε κρόνου παῖς ἀγκυλομήτεω
Ἢ ναύλῃσι τέρας, ἢε ςρατῷ εὐρεῖ λαῶν,
Λαμπρὸν, τοῦδέ τε πολλοὶ ἀπὸ σπινθῆρες ἵενται.
Τῷ εἰκυῖα πίπτεν ἐπὶ χθόνα Παλλὰς Ἀθήνη (27).

Qualem autem ſtellam mittit Saturni filius verſuti,
Aut nautis portentum, aut exercitui lato populorum,
Splendidam, unde multæ ſcintillæ emittuntur.
Huic ſimilis, impetu ferebatur in terram Pallas Minerva.

(27) *Iliad.* iv. *v.* 75.

Some,

Some, as Euſtathius (28) himſelf, take this to be a deſcription of a comet; and the juſtneſs of it will be acknowleged by all that remember the late one in 1743. By the beauty and livelineſs of the deſcription, likewiſe, one would be induced to believe farther, that it muſt have been the deſcription of one ſeen by Homer himſelf. But if the comet that appeared in 1681. hath a period of about 575 years (29), as it ſeems to have, we ſhall find, by counting backwards, that it muſt have viſited the earth about the year before Chriſt 619. at which time Homer might have been alive, and old enough to remember the terror and conſternation that it cauſed.

Another remarkable paſſage there is, in the *Odyſſy* (30), where, juſt before Ulyſſes recovered his wife and kingdom, the poet tells us, that

——Ἥλιος δὲ
Οὐρανῷ ἐξαπόλωλε, κακὴ δ' ἐπιδέδρομεν ἀχλύς

——*Sol quoque*
Ex cælo periit; ominoſaque ingruit caligo.

Ταῦτα δὲ ὡς ἀπὸ ἡλίου Ἐκλείψεως, ſays Euſtathius there: And again, Ἡ δὲ τοῦ Ἡλίου Ἔκλειψις οὐκ ἀπίθανος οἷα γενομένη ἐν Νουμηνίᾳ.

What authority Euſtathius had for ſuppoſing that this tranſaction was at the new moon, I know not. I

(28) Ἀστέρα δὲ νῦν ὁ πᾶς κυρίως λέγει, ἀλλὰ τὶ ἀστεροειδὲς οἷον Κομήτην ἢ σκιῶδες εἶδος.

(29) See Dr. Halley's *Aſtron. Tables*, or *Miſcel. Curioſ.* Vol. II.

(30) Lib. XX. v. 356.

think it no-where appears from the poet himſelf. That it is a deſcription of ſome total eclipſe, is, however, not improbable: And tho' an eclipſe, at the time Homer is ſpeaking of, ſeems purely poetical; yet the great eclipſe of the Sun, in the year before Chriſt 603. that parted the Lydian (31) and Median armies, muſt have made ſtrong impreſſions on every Ionian's mind, that ſaw it, and may be here very beautifully introduced.

I deſire no greater ſtreſs may be laid on theſe paſſages than they will bear: But I obſerve, that, in placing the age of Homer thus, we ſhall be enabled farther to ſolve a difficulty mentioned by Strabo (32). For this curious and accurate geographer and hiſtorian remarks, that Homer no-where mentions the empire of the Medes, nor the cities of Babylon and Tyre. But this laſt city was taken by Nebuchadnezzar, after a long ſiege (33), about the year before Chriſt 593. and was to continue, according to Iſaiah (34), in a low deſpicable condition 70 years; and therefore, probably, did ſo the greateſt part of Homer's life-time. The city of Babylon was, I think, taken by the

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(31) *Herodot. p.* 29. Edit. *Gronov.* See Maier's *Chronolog. Scythic.* in the *Act. Petropolit.* Tom. III. and what I have ſaid in the *Philoſ. Tranſ.*

(32) Ομηρος γοῦν ὅτε τὴν Τύρων ὅτε τὴν Μήδων ἀρχὴν οἶδεν· οὐδὲ γὰρ ἂν Θήβας Αιγυπτίας ὀνομάζων, καὶ τὴν ἐκεῖ, καὶ τὴν ἐν Φοινίκῃ πλουτον, τὴν ἐν Βαβυλῶνι καὶ Νίνῳ καὶ Ἐκβατάνοις παρεσιώπησε. Pag. 1068. Οἱ μὲν οὖν ποιηταὶ, τὴν Σιδόνα τεθρυλλήκασι μᾶλλον· Ομηρος δὲ οὐδὲ μέμνηται τῆς Τύρου. Pag. 1097.

(33) Ezek. xxix. 18. Επὶ Ιθωβάλου τοῦ βασιλέως ἐπολιόρκησε Ναβουχοδονόσορος τὴν Τύρον ἐπ' ἔτη δεκατρία. *Joſeph. cont. Apion. p.* 1344. *Ed. Hudſ.*

(34) Chap. xxiii. ver. 15.

Medes

Medes about the year before Chriſt 558. and about four years afterwards the Median empire itſelf was put an end to by Cyrus and his Perſians (35). Within this period of time, therefore, it is now farther probable, from this obſervation of Strabo, that both the *Iliad* and *Odyſſy* were compoſed.

And this, again, will receive an additional confirmation, by conſidering the following lines of the *Odyſſy* (36) itſelf; where Eumæus tells Ulyſſes, that

Νῆσός τις Συρίη κικλήσκεται εἴπου ἀκούεις
Ὀρτυγίης καθύπερθεν, ὅθι ΤΡΟΠΑΙ ΗΕΛΙΟΙΟ

Inſula quædam Syria vocatur, ſicubi audis
Trans ortygiam; ubi MUTATIONES SOLIS.

But what is to be underſtood by the words τροπαὶ ἠελίοιο, or *mutationes Solis*, as the tranſlator renders them? The word *tropics*, we know, is ſometimes uſed for thoſe points of the ecliptic through which the ſolſticial colure is drawn; but this cannot be the meaning of it here, as it is impoſſible that the tropics, in this ſenſe, ſhould be at the iſland Syra, or Syria. This iſland is one of the Cyclades, and lies, according to the beſt modern geographers, in latitude 37° 25′ north; where, conſequently, the height of the equator is 52° 35′, and the Sun's zenith diſtance, on the day of the ſummer ſolſtice, 13° 56′. Homer, therefore, could not mean, likewiſe, that this iſland

(35) The proof of this being too long for a note, is conſidered, at large, in a treatiſe by itſelf.

(36) Lib. xv. ver. 402.

lay

lay under one of the tropics, much leſs that it lay under both.

Another ſignification of the word *tropic* is, when it is uſed for that moment of time when the Sun, by his apparent motion, enters either of the ſolſticial points: But neither could Homer uſe the word here in this ſenſe. For the ſolſtice, according to this meaning of the word, is not only at the iſland Syria, but every-where elſe; and is only ſooner or later, in time, as places lie to the eaſtward or weſtward of each other. For if the time of the ſummer ſolſtice, this year, is at 12 o'clock at noon, as reckoned at Greenwich, it will only be 11 o'clock to places that lie in 15° of weſtern longitude; or 1 o'clock in the afternoon to ſuch as lie in 15° eaſtern longitude from it.

The only remaining ſenſe, then, of which the words τροπαὶ ἡελίοιο ſeem capable, is, as far as I can apprehend, by ſuppoſing that they mean ſome inſtrument or other, as a gnomon, or the like, erected there; which, by the increaſing or decreaſing lengths of its meridional ſhadows, pointed out the days of the ſolſtices: I ſay the days; becauſe, if thoſe could be obtained, it was a degree of accuracy as great, I ſuppoſe, as obſervations of this ſort could, in thoſe times, pretend to.

And that we are not much miſtaken in apprehending this to have been an inſtrument of this ſort, may be gathered, perhaps, from Diogenes Laertius. For, in his life of Pherecydes, who was a native, at leaſt an inhabitant, of this very iſland, he ſays, Σώζεσθαι δὲ καὶ ἡλιότροπιον ἐν Σύρᾳ τῇ νήσῳ, *ſervatur & heliotropium in Syra inſula.*

Theſe

Theſe words Aldobrandinus (37) ſuſpects to be an interpolation. *Quid enim*, ſays he, *ad Pherecydem heliotropium?* But if we read the paſſage thus, as, probably, it ſhould be read, σώζεται ὃ καὶ ἡλιοτρόπιον ΑΥΤΟΥ ἐν Σύρᾳ τῇ νήσῳ; the whole will be conſiſtent and pertinent to Pherecydes, and likewiſe be a direct proof of ſuch an inſtrument being there, as we have ſuppoſed.

Peter Huet (38), it is true, ſuſpects, from the paſſage in the *Odyſſy* above-quoted, that this inſtrument was repaired only by Pherecydes there, and not firſt erected by him. But as this reaſoning depends only on the ſuppoſed much greater antiquity of Homer, the very point in queſtion, we are equally at liberty to ſuppoſe the contrary; and that Pherecydes was the original erecter of it in the iſland. And that this conjecture may not paſs unſupported, it may be obſerved, that, according to Laertius (39), Anaximander, who lived about the ſame time, was the firſt inventer of the gnomon; or, rather, the introducer of it at Lacedæmon.

Pherecydes, according to Laertius, flouriſhed about Olymp. LXIX. or the year before Chriſt 500. and Anaximander, he ſays, was 64 years old Olymp. LVIII. 2. or the year before Chriſt 543. and was, therefore, ſomething older than the former, if the numbers in Laertius here may be depended on, a thing not always to be done.

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(37) In Loc.

(38) And ſo Bochart. *Geog. Sacr.* Part II. l. i. c. 14. Vid. *Menag. Obſervat. in Loc.*

(39) In Vit. ejuſ.

The

The pole, the gnomon, and the diviſion of the day into 12 parts, are expreſly ſaid by Herodotus (40) to have come from the Babylonians to the Greeks; and it is more natural to ſuppoſe, from the uſual progreſs of ſcience, that the iſlands neareſt to the Aſiatic coaſt were acquainted with theſe improvements before Peloponneſus, and the places more remote from thence. Pherecydes, therefore, it is probable, erected his gnomon at Syra ſomewhat earlier than Anaximander did his at Lacedæmon. But as we read of nothing of this kind among the Greeks before their time, we may conclude them to have been totally ignorant of theſe inventions as early as the year before Chriſt 610. when, if Laertius ſays true, Anaximander was born. But in the year before Chriſt 558. as before obſerved, Babylon was taken by the Medes; and it may be no abſurd conjecture to imagine that ſuch Chaldeans, as were forced from their native country by their enemies, and ſheltered themſelves among the Ionians, firſt taught them, and by their means the reſt of the Greeks, their aſtronomical diſcoveries. It is certain, that the taking of Conſtantinople by the Turks hath had a like effect in later times (41).

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(40) Πόλον μὲν γὰρ καὶ γνώμονα, καὶ τὰ δυώδεκα μέρεα τῆς ἡμέρης παρὰ Βαβυλωνίων ἔμαθον Ἕλληνες. Pag. 127. *Edit. Gronov.* Πόλος dicebatur, quod poſtea ὡρολόγιον, ſays Scaliger on *Manil.* p. 254. And ſo Athenæus Deipnoſoph. l. v. ſpeaks of Πόλον ἐπὶ τῶ καλᾶ τὴν Ἀχραδινὴν ἀπομεμιμημένον ἡλιοτροπίῳ. And Ariſtophanes in γηρυτάδῃ ſays, Πόλος τὸ δὴ ἔσιν· Ἐλασαποσὴν ἥλιος τέτραπται. And Jul. Pollux ſays, *Cottabium*———ἐώκει δὲ πόλῳ τῇ τὰς ὥρας δεικνύσῃ. L. vi. cap. 19.

(41) The Turks ſate down before that place April 4. 1453. and, when they took it, deſtroy'd 120000 volumes. See *Hod.* de *Græcis Illuſt.* &c. p. 192.

How

How long before this the Babylonians themſelves were acquainted with the uſe of the gnomon, is unknown, the Greeks being poorly informed, as to the hiſtory of them, in theſe early times; and the Jews, the only people beſides whoſe hiſtory of them is extant, applied themſelves but little to ſcience at home, and, by their conſtitution, had but a ſmall connection with their neighbours abroad. With regard to the ſcience of aſtronomy, in particular, it muſt have been, as then taught and practiſed, in a manner, forbidden them, as it was nothing more than genethliacal aſtrology; a thing vain and futile in itſelf, if not impious.

Among the Jews, however, we find, under the reign of Hezekiah, that the Sun is ſaid to have gone back 10 degrees on the dial of Ahaz (42). What the

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(42) *Iſa.* xxxviii. 8. and 2 *Kings* xx. 11. In the Hebrew, the words are only מעלות אחז, the laſt of which words the LXX render ἀναβαθμὸς; the Arab. درجات; the Vulgate by *Linea*, *Horologium*, and *Gradus*. The Chaldee Paraphraſt labours under equal perplexity. Rabbi Kimchi, on 2 *Kings* xx. renders מעלות by אבן מכוונת לדעת שעות היום, *a ſtone erected to tell the hours of the day by*. And to the ſame purpoſe R. Solom. Jarch. on *Iſaiah*. It may not be improper to add in this place, perhaps, that as the 11 *ſtars* correſpond to Joſeph's 11 *brethren*, *Gen.* xxxvii. 9. and the 3 *branches*, and 3 *baſkets*, reſpectively denote 3 *days*, in ch. xl. 12. 18. and 7 *kine*, and 7 *ears of corn*, repreſent 7 *years*; ſo, in the paſſage before us, the number of מעלות ought, for the ſame reaſon, to correſpond with the years added to Hezekiah's life. If 15 years were added to his life, then muſt the ſhadow have gone back 15 *maaloth*; but if the ſhadow went back only 10 *maaloth*, then muſt 10 years only have been added to his life. The numbers in this place, therefore, are, ſome how, or other, undoubtedly corrupted. I am inclined, then, to think, that, inſtead of *I will add to thy life* 15

the form of this dial was, is unknown; but it may not be improbable, that it was copied from the Babylonians, as that prince ſeems to have been curious and fond of exotic cuſtoms. This was about the year before Chriſt 724. and, conſequently, 166 years before Babylon was taken, and 114 before Anaximander was born.

I call this a dial, in compliance with cuſtom, and for want of a better term to expreſs it by; tho' it was, probably, nothing more than a gnomon erected perpendicular to the plane of the horizon; and ſerved not only for diſtinguiſhing the different parts of the day, but in a rude manner, likewiſe, the times of the ſolſtices and equinoxes. For the ingreſs of the Sun into the four cardinal points might be thought, by the aſtrologers, to have been of as much conſequence, in reſolving genethliacal queſtions, as knowing the time of the day: I ſay the time of the day; becauſe the hours marked out, by inſtruments of this kind, were not equable, or equinoctial hours, but popular; being longer or ſhorter, in any aſſigned place, according to the different ſeaſon of the year.

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15 *years*, it ſhould be, *I will add to thy life* 10 *years*; the ſhadow going back, to denote this, only 10 *maaloth*. For as the Babylonian and Jewiſh day conſiſted only of 12 hours, it is highly probable, that, on theſe kind of inſtruments, there were no more than 12 *maaloth*. The ſhadow, therefore, could not go back 15 *maaloth*; nor, conſequently, agreeable to the rule before laid down, pertinently repreſent the addition of 15 years. It was to this diviſion of the day into 12 parts that Craſſus alluded, when he ſaid to king Deiotarus, ‘ Quid hoc rei eſt, duodecima jam tibi tantum non in-
‘ ſtat hora, & novam nihilominus urbem ædificare pergis?’ *Cæl. Rhodigin.* p. 318.

How

How early the Greeks had the uſe of the word ὥρα, is not agreed on. The Babylonians, as we learn from the book of *Daniel* (43), uſed the equivalent word זמנא at leaſt as early as the reign of Nebuchadnezzar, or the year before Chriſt 616. when Jeruſalem was firſt taken by that prince, and 6 years before the birth of Anaximander. But that the diviſions on his, or Pherecydes's dial, were called ΩΡΑΙ, is not ſo clear.

Salmaſius (44) ſays, the word was unknown to the Greeks for more than two hundred years after the death of Anaximander: And farther, that it is never uſed by Plato, Ariſtotle, Theophraſtus, nor any author of that age; nor even by Menander, or any other writer of the *new Comedy* after the time of Alexander the Great.

But, with deference to this opinion of his, it may be obſerved, that there is a paſſage in Xenophon (45) where the word Ωρα ſeems uſed in the ſenſe contended for. Ουκοῦν χὴ ἐπειδὴ ὁ μὲν ἥλιος (ſays Socrates) φωλεινὸς ὢν τὰς τὲ ΩΡΑΣ τ̃ ἡμέρας ἡμῖν χὴ τἄλλα πάνλα σαφηνίζει· ἡ δ' νύξ, διὰ τὸ σκολεινὴ εἶναι ἀσαφες́ερα ἐς́ιν, ἄς́ρα ὃν τῇ νυκλὶ ἀνέφηναν, ἃ ἡμῖν τὰς ΩΡΑΣ τ̃ νυκλὸς ἐμφανίζει, χὴ διὰ τ̃ο πολλὰ ὧν δεόμεθα πράλο-

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(43) *Dan.* iii. 15. iv. 16.

(44) Certe novæ comediæ ſcriptores, quorum princeps Menander, qui poſt Alexandrum magnum vixerat, nuſquam τ̃ ὥρας meminere pro diei particula, ut grammatici nobis veteres teſtantur. Sed nec ea vox hoc ſenſu apud Platonem, Ariſtotelem, Theophraſtum, aut alios æquævos ſcriptores uſpiam legitur. *Plinian. Exercit.* p. 633.

(45) *Memorabil.* l. iv. cap. 3. ſect. 4. And ſo Herodotus, before him, uſeth the word, p. 529.

μεν. *Quia vero Sol lucidus eſt, ac nobis* HORAS *diei, aliaque omnia clare demonſtrat, nox autem propter tenebras eſt obſcurior, aſtrorum lucem noctu protulerunt* (Dii) *quæ nobis* HORAS *noctis indicarent; quo fit, ut multa tum quorum opus eſt perficiamus.* But Xenophon flouriſhed, according to Laertius (46), about Olymp. XCIV. 4. or the year before Chriſt 397. and about 148 years after the death of Anaximander; at which time, it ſeems now, the Greeks were acquainted with the word ὥρα.

But tho' the word ὥρα itſelf could not be proved to have been in uſe among them at this time, yet it ſeems as if they had what was equivalent to it before: For according to Menander, as cited by Julius Pollux (47), what was then called ὥρα, *an hour*, and ἡμιώριον, *half an hour*, was called παρὰ τοῖς παλαιοῖς *by the antients*; Σημεῖον, *a mark*: And the reaſon, I ſuppoſe, was, becauſe the ends of the ſhadows were marked with the letters of the alphabet, called Στοιχεια, *elements*, as their lengths were meaſured by feet. And as the day was divided into XII parts, ſo the greateſt length of their ſhadows were XII feet; the Sun being after this, in the evening, and before this, in the morning, too low to make any farther meaſures uſeful.

Inſtances of what hath here been ſaid, are eaſily to be met with in the comic writers. Thus in Ariſtophanes's *Concionatrices* (48).

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(46) *In Vit. ejuſ.*
(47) Pag. 47. *Edit. Kuſter.*
(48) Pag. 457. *ibid.*

——σοὶ ϑ᾽ μελήσει
Ὁλαν ἢ ΔΕΚΑΠΟΥΝ τὸ ΣΤΟΙΧΕΙΟΝ λιπαρῶς
χωρεῖν ἐπὶ δεῖπνον.

——*tibi autem curæ erit*
Quando fuerit decem pedum elementum pinguè
(unctum) ire ad cænam.

Upon which word ΔΕΚΑΠΟΥΝ, the ſcholiaſt obſerves, ἡ τῦ Ηλίυ σκιὰ ὅλαν ἢ δέκα ποδῶν· Θέλει ἓν εἰπεῖν ὅλε γίνεσθαι ὀψέ. Αλλως τὸ παλαιὸν καλῦνλες ἐπὶ δεῖπνον κ᾽ καλέμενοι παρεσημάινονλο τ᾽ σκιάν, κ᾽ ὅλως οἱ μεν ἔμενον τῆς κλησένλας, οἱ ϑ᾽ ἀπήεσαν ἐπὶ τὰς ἑςιάσεις, οὐδέπω τηρήσεως (Vid. *Caſaub.* in *Athenæum*, p. 425. 49.) ἦσης ἑλέρας, ἀφ᾽ ἧς οἷόν τε ἦν τεκμήρασϑαι εἰς πόσας ὥρας τροπήκει. *Quando Solis umbra ſit decem pedes longa*, q. d. *quando ſero ſit. Aliter; antiquitus ad cænam invitantes & invitati umbram notabant, unde hi invitatos manebant, illi ad convivia ſe conferebant, quum nulla alia eſſet obſervandi ratio, qua indicium ſumerent quot horas jam dies declinaſſet.* And Heſychius likewiſe informs us, that τοῖς ποσὶ καλεμέτρουν τὰς σχιὰς ἐξ ὧν τὰς ὥρας ἐγίνωσκον.

And that twelve was the whole graduation, and twelve feet the longeſt ſhadow in dials of this form, may be collected, I think, from this epigram, cited by Salmaſius, on Solinus (49):

Ωράων σκοπιάζε σοφὸν σημάνορα χαλκὸν
Αυλῆς ἐν ΜΟΝΑΔΟΣ μέχρι ΔΥΩΔΕΚΑΔΟΣ.

(49) *Plin. Exercit.* p. 634.

And

And before this, in a fragment of Menander (50), it is ſaid,

——διαφέρει Χαιρεφῶντος οὐδὲ γρὺ
Ανθρωπος ὅστις ἐστὶν ὃς κληθεὶς πολὺ
Εις ἑστίασιν ΔΩΔΕΚΑΠΩΔΟΣ ὄρθρου
Πρὸς τὴν Σελήνην ἦλθε τὴν σκιὰν ἰδὼν
Ως ὑστερίζων καὶ παρὼν ἅμ' ἡμέρᾳ

——*a Chærephonte nullo modo*
Homo differt, quiſquis eſt, qui vocatus aliquando
Ad convivium, cum umbra decem pedum foret, ſummo mane
Ad Lunam cucurrit conſpicatus umbram
Quaſi æquo diutius moratus, & adfuit una cum die.

And Heſychius, on the word ΔΩΔΕΚΑΠΟΔΟΣ, ſays, ὅλως ἔλεγον ἐλλειπτικῶς στοιχείου ἢ σκιᾶς· ἔτω γὰρ συνελάμβανον ἐπὶ δεῖπνον ἥξειν τοῦ στοιχείου ὄντος ΔΩΔΕΚΑΠΟΔΟΣ, ὡς νῦν πρὸς ΩΡΑΣ φασί. *Ita dixerunt, ſubintellecto elemento vel umbra. Sic enim ad cænam conventuros pacti ſunt olim exiſtente elemento duodecim pedes longo, ut nunc ad horas fit.*

I have dwelt the longer on this head, as it helps us to form ſome judgment on the nature of all theſe dials, as well that of Ahaz, as thoſe of Pherecydes and Anaximander; the מעלות of the firſt being naturally very capable of being mark'd ΣΤΟΙΧΕΙΟΙΣ on the other (51).

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(50) *Menand. Reliq.* p. 139. *Edit. Cleric.*

(51) The Hebrew word comes from עלה, *ſcandit, aſcendit*; and the Greek word from ΣΤΕΙΧΩ, *eo, vado*, &c.

But

But to return: If the Greeks were not acquainted with this invention of the Babylonians earlier than the year before Chriſt 610—If Pherecydes about that time firſt ſet up his dial in the iſland Syra, and Homer alludes to it in his *Odyſſy*, as ſeems highly probable; then muſt he, and conſequently Heſiod, if contemporary with him, not be older than what we above have made him.

However ſtrange this argument, drawn from the dial of Pherecydes, may appear to ſome, yet that I am not ſingular in it, is evident, from this note of Barnes upon the place: 'Qui hæc de heliotropio 'ſumunt, *ſays he*, parum vident, aut plus ſatis; quod '& illud a Pherecyde inventum, atque proinde Ho-'mero parem, aut priorem allucinantur, Cl. Dod-'velli rationes nihil faciunt: cum Lycurgus, qui ip-'ſas Olympiades præceſſit, Homeri opera, a Creo-'phyli Samii poſteris excepta, in Græciam primus 'intulerit, ut Heraclides & Plutarchus in Lycurgo.'

That Lycurgus is commonly placed before the Olympiads, is true; but the hiſtory and chronology of that lawgiver is not ſo certain as to leave no room to ſuſpect the contrary. Mr. Dodwell, whoſe ſkill in chronology was vaſtly ſuperior to that of Barnes, ſays, there are very good reaſons for ſuppoſing him to be later (52): And with him agrees Sir Iſaac Newton (53).

As to the aſſertion of Plutarch, it may be obſerved, from Strabo (54), that, according to ſome,

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(52) *De Cyc. Vet.* p. 131.
(53) *Chronol.* p. 126.
(54) *Geograph.* p. 739.

Lycurgus

Lycurgus himſelf had an interview with Homer in the iſle of Chios; and Plutarch, likewiſe, was no ſtranger to the ſame report (55).

As a farther confirmation, however, that we are not very wrong in placing the age of theſe two poets as we have done, it may be remarked, that, in the deſcription given by Heſiod of lucky and unlucky days, he tells us, τετρὰς μηνὸς ἀρίστη (56). But the firſt perſon, among the Greeks, that called the laſt day of the month by that name, or that uſed the word ΤΡΟΠΑΙ, if we believe Laertius, was Thales. Neither Homer nor Heſiod, therefore, if this obſervation be true, can be older than Olymp. XXXV. 1. or the year before Chriſt 637. when that philoſopher was born. But as it muſt have been ſome time before he could apply himſelf to aſtronomical ſtudies, and probably not till the middle part of his life, or about the year before Chriſt 600. the *Odyſſy* could not well have been compoſed before.

But Piſiſtratus, as we are informed by Tully (58), firſt collected Homer's verſes, and digeſted them in the manner we now have them. And Solon, according to Laertius (59), proved the right of the Athenians to the iſland Salamis, from theſe lines of the *Iliad:*

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(55) *Vit. Lycurg.*

(56) *Dierum.* v. 2.

(57) *In Vit. ejuſ.*

(58) Qui primus Homeri libros, confuſos antea, ſic diſpoſuiſſe dicitur, ut nunc habemus. *De Oratore*, l. iii. Πεισίστρατος συναγαγὼν ἀπέφηνε τὴν Ἰλιάδα καὶ Ὀδύσσειαν. *Ælian. Var. Hiſt.* l. xiii. c. 14.

(59) *In Vit. ejuſ.*

Αιας δ' ἐκ Σαλαμῖνος ἄγεν δυωκαίδεκα νῆας
Στῆσε δ' ἀγῶν ἵν' Αθηναίων ἵππλοι φάλαγγες.

Solon, according to Laertius, flouriſhed about Olymp. XLVI. and in the 3d year of it was archon, and publiſhed his laws. This was the year before Chriſt 590. What his age was at that time, he doth not tell us, but that he was 80 at his death; which by Plutarch, in his life of that lawgiver, is placed Olymp. LIII. 3. or the year before Chriſt 562. If ſo, he muſt have been about 52 the year that he was archon. And that he could not have been very young then, is plain, from the poſt and credit he was in.

Upon the expiration of his archonſhip, as we are informed by Plutarch, he travelled for 10 years, and returned an old man, as indeed he was, being now about 62 years of age: This was the year before Chriſt 580. During this interval, it is highly probable, he had his interview with Crœſus (60), and brought back with him, to Athens, Homer's poems, which he might meet with at Smyrna, or ſome other of the Ionian cities. Upon his return, he found his country torn with factions, and that Piſiſtratus had formed the deſign of making himſelf maſter of the ſtate, which he ſoon afterwards effected. What year

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(60) *Herodot.* p. 11. *Edit. Gronov.* Τὴν ὦ πρὸς Κροῖσον ἔντευξιν αὐτῷ δοκέουσιν ἔνιοι τοῖς χρόνοις ὡς πεπλασμένην ἐλέγχειν. Ἐγὼ ὦ λόγον ἔνδοξον οὐδένα, καὶ τοσούτους μάρτυρας ἔχοντα, καὶ (ὁ μεῖζον ὄγκος) πρέποντα τῷ Σόλωνος ἤθει καὶ τῇ ἐκείνου μεγαλοφροσύνης καὶ σοφίας ἄξιον, οὐκ ἀπιστέω. προήσεως χρονικοῖς τισι λεγομένοις κανόσιν, οὓς μυρίοι διορθοῦντες ἄχρι σήμερον οὐδὲν ἀτρεκὲς ὁμολογούμενον δύνανται καταπῆξαι τὰς ἀνακολουθίας. *Plutarch. Vit. Solon.*

this was in, is uncertain. The Oxford marble (61) placeth it, as doth Plutarch in the archonſhip of Comeas, which is ſuppoſed to concur with Olymp. LIV. 4. or the year before Chriſt 557. But Tatian (62), Clemens Alexandrinus (63), and Scaliger (64), among the moderns, fix the government of Piſiſtratus to Olymp. L. or 577 years before Chriſt. And this, indeed, agrees beſt with Plutarch; who ſays, that Piſiſtratus, after ſeizing the adminiſtration, 'honoured 'and eſteemed Solon, and often ſent for him, and ad-'viſed with him.'

In what year Piſiſtratus digeſted Homer's poems, is not ſaid; but it was, moſt probably, ſome time, or other, while he was in credit; and therefore, it is likely, about this very year 557. before Chriſt.

In the years of Solon's life, and Piſiſtratus's government, I have hitherto followed the chronology of the Greeks; which, however, I am apt to think, placeth them both ſomewhat higher than they ought to be; a fault not to be corrected in this inſtance alone.

It is natural to aſk, what could induce Solon and Piſiſtratus, whoſe ſchemes of politicks were ſo widely different, to concur in recommending and encouraging the ſinging Homer's works. If the beauty and elegance of the compoſition alone be thought a ſufficient anſwer, it muſt be obſerved, that ſuch diſtinguiſhing care of them, ſhewn by two ſuch able

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(61) ΑΦΟΥ ΠΕΣΙΣΤΡΑΤΟΣ ΑΘΗΝΩΝ ΕΤΥΡΑΝΝΕΤΣ ΕΝΕΤΗ ΗΗ[Δ]ΔΔΔΔΠΙΙ ΑΡΧΟΝΤΟΣ ..... ΙΚ .... ΟΥ. N° 56. where ſee the Commentators.

(62) Contra *Græcos*.

(63) *Stromat*. l. i.

(64) In *Euſeb*.

ſtateſmen,

ſtateſmen, ſeems to intimate ſome deeper views than the world hath hitherto been apprifed of.

Auguſtus, it is ſaid, ſet a very high value on the *Æneid*; and the deſign of the poet in compoſing it is well known; but the drift of the *Iliad*, I think, hath not been ſo well agreed on.

The Trojan war, as the moſt judicious of the Greek hiſtorians (65) informs us, was in itſelf nothing near ſo conſiderable as the poets had made it. But for what end was this? Was it the ſports of the imagination only? Were heaven and earth armed for nothing more than the writer's fancy, and the reader's amuſement? Something more intereſting, ſure, was at the bottom of all this machinery; and, if I am not much miſtaken, the very circumſtances of the times, we are now ſpeaking of, naturally gave birth to ſuch a poem as the *Iliad*.

The Perſian empire, by the conqueſts of Cyrus, was growing very extenſive and formidable, and muſt, conſequently, greatly alarm the Ionians, who might juſtly apprehend their ſharing the ſame fate with the Aſſyrians, Medes, and Lydians. That he had formed a deſign of invading them, appeared, as we are informed by Herodotus (66), from the anſwer he gave their ambaſſadors. This they could not but ſee, and at the ſame time perceive themſelves unable to op-

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(65) Καὶ αὐτά γε δὴ ταῦτα ὀνομαζόταται τῶν πρὶν γενομένων μᾶλλόν ται τοις ἔργοις ἐπιπροσέσεσθαι ὄντα τῆς ὀρθῆς, καὶ τῶν νῦν περὶ αὐτῶν διὰ τὸν ποιητὰς λόγον κατεσχηκότων. *Thucyd.* l. i. ſect. 11.

(66) The paſſage is too long to be tranſcribed. See *Herod.* p. 58. *Edit. Gronov.* and *Thucyd.* lib. i. ſect. 16.

poſe him, unleſs by a timely union among themſelves, and with the reſt of the Greek ſtates in Europe, and the iſlands adjacent to the Aſiatic coaſt. Such a confederacy had formerly ſubſiſted, and Aſia had felt the effects of it in the deſtruction of one of its ſtates. This, indeed, was the work of a ten years war; but that, on the other hand, was owing only to the quarrels and diſſentions of the princes engaged in it; a leſſon very proper to be inculcated at this juncture, when they were to fight in the cauſe of liberty, when they might expect the ſame gods would be on their ſide as formerly, and had ſtronger motives to unanimity in their councils, than when they were only revenging the injuries offered to a ſingle family.

To promote ſuch a confederacy as this, appears to me to be the plan of Homer's *Iliad*. This, as a bard, he was employed to ſing at feaſts and entertainments; and the introducing and encouraging ſuch a poem by Lycurgus at Sparta, and Solon at Athens, was every way worthy the character and wiſdom of thoſe lawgivers.

But if ſuch a confederacy could once be formed, it was plain, the Athenians, the moſt conſiderable of their ſtates at that time, would bear the greateſt ſhare in it: Whoever, therefore, was maſter of Athens, would, of courſe, be at the head of the whole alliance. Without ſuch a head, and furniſhed with proper authority to command obedience, former experience had taught them what great diſadvantages muſt unavoidably ariſe to the common cauſe. Therefore,

Ουκ ἀϛαθὸν Πολυκοιρανίη, ΕΙΣ Κόιρανος ἔϛω,
Εις ΒΑΣΙΛΕΥΣ, ᾧ ἔδωκε Κρόνου παῖς ἀγκυλομήτεω
Σκῆπτρον τ' ἠδὲ θέμιϛας, ἵνα σφίσιν βασιλεύη (67).

Is it at all ſurpriſing, then, that Piſiſtratus, whoſe abilities and intereſt appear to have been very great, ſhould ſeize the government of Athens at this time? And doth there not appear the higheſt reaſon in the world why he, as well as Solon, ſhould take ſuch particular care of Homer's poems. Upon the whole, then, I think, it may be concluded, with a good degree of probability, from what hath been here laid down, that the *Iliad* and *Odyſſy* were both compoſed about the time of Cyrus, or the year before Chriſt 558. if, as the antients generally do, we make his reign to commence from his taking of Babylon.

And ſince thoſe that make Heſiod the oldeſt of the two poets, place him but a few years earlier than Ho-

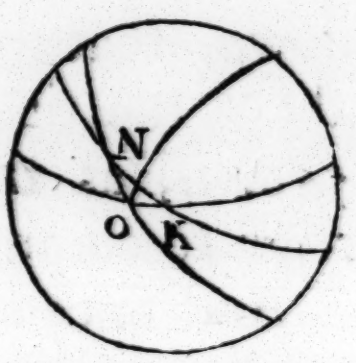
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(67) *Iliad.* ii. *v.* 204. It was natural for the Ionians to apply themſelves to the Athenians, as being the largeſt maritime power, and becauſe, as Thucydides informs us, Ἰωνας μεν Αθηναῖοι και Νησιωτῶν τὰς πολλὰς ὤιησαν. Pag. 11. *Edit. Waſſ.* See, likewiſe, *Meurſ. de Fort. Athen.* c. 6. Herodotus ſays, that, upon this occaſion, all the Ionians, except the Mileſians, met in their common council called παναιώνιον; and that ἐδοξε κοινῶ λόγῳ πέμπειν ἀγγέλους ες Σπάρτην δεησομένους Ἰωσι τιμωρέειν. The reaſon, perhaps, of their ſending to Sparta, was, to engage the Peloponneſians; that being not only the principal city there at that time, but, likewiſe, having an old quarrel with the Aſiatics ever ſince the Trojan war. Why he hath not mentioned their ſending to the Athenians, is not very evident: Perhaps the members of this council, out of hatred to the reſt of the Greek cities in their neighbourhood, planted by the Athenians, refuſed to ask their aſſiſtance: And this reaſon Herodotus himſelf ſeems to help us to. Οι μὲν νῦν ἄλλοι Ἰωνες καὶ Αθηναῖοι ἐφυγον τὸνομα ὁ βυλόμενοι Ἰωνες κεκλῆσθαι, ἀλλὰ καὶ νῦν φαίνονται μοι οἱ πολλοὶ αὐτῶν ἐπαισχύνεσθαι τῷ ὀνόματι. Pag. 59.

mer,

mer, not enough, however, to cauſe any obſervable change in the riſing of the fixed ſtars; we may take the difference, at a medium, at 20 or 22 years; which will bring us to the year before Chriſt 580. for the time when Heſiod flouriſhed.

Nor will the argument from aſtronomy be at all inconſiſtent with this determination. For in the year before Chriſt 580. the time of the winter ſolſtice was December 27. in the morning; and 60 days after that will bring us to February 25. when the Sun's true place, at noon, was 10ˢ 29° 47′ 30″, his declination ſouth 11° 33′ 56″, and his right aſcenſion 331° 53′ 53″. Then, in the figure here,



Tan. NO	=	11 33 56	9.3109992
—R + cot. NKO	=	51 55 00	9.8941114
KN	=	9 13 45	9.2051106

= the Sun's aſcenſional difference.

From the year before Chriſt 580. to the end of the year after Chriſt 1689. are 2269 years; the preceſſion for which time is 1ˢ 1° 30′ 50″; which, ſubtracted from 6ˢ 19° 53′ 52″, as before, gives the place of Arcturus, the year before Chriſt 580. = 5ˢ 18° 23′ 2″; and the angle DCE, in the 2d figure, = 78° 23′ 2″. Then

Rad. + cof. DCE	=	78 23 02	— 19.3039589
— Cot. DC	=	23 29 00	— 10.3620437
Tan. CE	=	5 00 00	— 8.9419152
PC	=	59 03 00	
Cof. PE	=	54 03 0	— 9.7686966

Cof.

Cof. PE = 54° 03′ 00″ — 9.7686966
Cof. DC = 23 29 00 — 9.9624527

19.7311493
Cof. DE = 5 00 00 — 9.9983442

Cof. DP = 57 16 55 — 9.7328051

the complement of which, = 32° 43′ 05″, = the declination of Arcturus.

Then, in 3d fig. Tan. MO = 32° 43′ 05″ — 9.8078283
— Rad. + cot. MKO = 51 55 00 — 9.8941114

Sin. KO = 30 13 35 — 9.7019397

= the aſcenſional difference.

Sin. PC = 59 03 00 — 9.9332931
Sin. DCP = 78 23 02 — 9.9910119

19.9243050
Sin. DP = 57 16 55 — 9.9249738

Sin. PDC = 86 49 20 — 9.9993312

the complement of which = 93° 10′ 40″. The right aſcenſion, therefore, of Arcturus then, was 183° 10′ 40″; from which ſubtracting the aſcenſional difference found above, gives the oblique aſcenſion of Arcturus = 152° 57′ 05″.

Then the ſemid. arc in a right ſphere = 90° 00′ 00″
Sun's aſcenſional difference 9 13 45

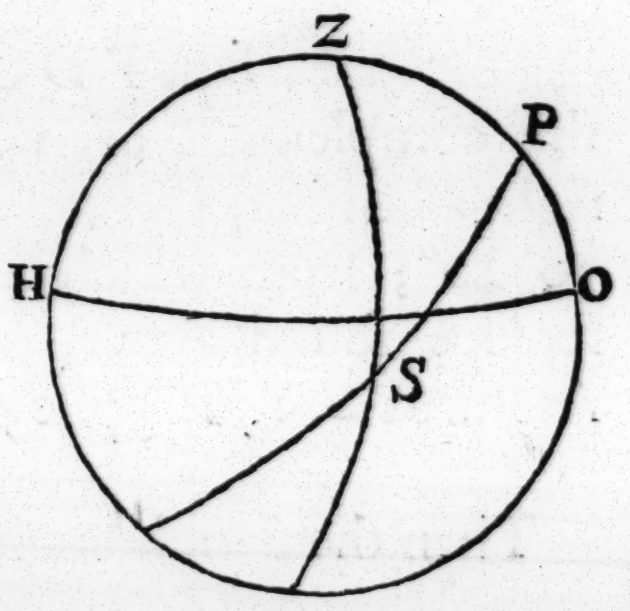
Semidiurnal arc 80 46 15

which,

which, converted into time, gives $5^h\ 23'\ 5''$; whence the nocturnal arc $= 13^h\ 13'\ 50''$.

	°	′	″
Sun's oblique aſcenſion	341	07	38
Oblique aſcenſion of Arcturus	152	57	05
Difference	188	10	33
	h	′	″
This, converted into time, gives	12	00	42
Nocturnal arc	13	13	50
Difference	1	13	08
Add ſemidiurnal arc	5	23	05
Time of the riſing of Arcturus	6	36	13
Time of Sun-ſetting at Athens	5	23	05
Arcturus therefore roſe, after Sun-ſet there,	1	13	08

Let us now ſuppoſe, farther, that twilight ends when the Sun is 18 deg. below the horizon; and therefore, in the figure, where H O repreſents the horizon, P O the height of the pole at Athens, we have Z P the complement of P O $= 51° 55'$, P S $=$ the diſtance of the Sun from the pole, $= 101° 33' 56''$, Z S $=$ the Sun's diſtance from the zenith, $= 108°$. Then,



Z S

	°	′	″		
ZS =	108	00	00		
PZ =	51	55	00	arith. complem. =	0.1039621
PS =	101	33	56	arith. complem. =	0.0089087
Sum =	261	28	56		
Half =	130	44	28	=	9.8794779
— ZS =	108	00	00		
Diff. =	22	44	28	=	9.5872258
				Sum =	19.5795745

Half Sum = 9.7897872 = the coſine of 51° 57′ 15″; the double of which is = 103° 54′ 30″ = ZPS; which, converted into time, gives 6h 55′ 38″ for the end of twilight. Since, therefore, Arcturus roſe at 6h 36′ 13″, and, conſequently, near 20′ before the end of twilight; it might then be ſaid very properly, in the popular and leſs determinate ſenſe of the word, to riſe ΑΚΡΟΚΝΕΦΑΙΟΣ.

From what hath been ſaid, my lord, doth it not ſeem pretty clear, that Homer and Heſiod both lived about the year before Chriſt 580. and that, as I ſaid, from ſeveral arguments of an aſtronomical nature? The only difficulty that, I think, can be made to this, is, how to reconcile it with the expreſs teſtimony of Herodotus to the contrary. In his life of Homer, as we have ſeen (68), he places him 622 years before the expedition of Xerxes into Europe; but in his hiſtory he ſays, both Homer and Heſiod were not

(68) Not. 3. & 4.

more than 400 years before his time; that is, ſince there were but 50 years between the Peloponneſian war and the battle at Salamis (69), little more than 450 years before the ſame expedition.

Scaliger, in his notes on Euſebius (70), corrects the former paſſage of Herodotus by the latter; and, inſtead of ἑξακόσια, reads τετρακόσια; which will place Homer about the year before Chriſt 902. conſiſtent enough with Paterculus and the marble (71), but different from his hiſtory by 71 years.

Whether this correction of Scaliger's be right, or not, I ſhall not here ſtand to enquire; but I am apt to think the word τετρακοσίοισι itſelf, in Herodotus, is corrupt.

The Greek chronology, like that of other nations, hath been generally carried up too high; the natural conſequence of ignorance, and a defect of memoirs. This is only now to be corrected by perſons of learning and abilities, capable of examining and comparing things with each other. In the time of Herodotus, no doubt, the popular accounts of Homer and Heſiod carried them up much beyond their proper time: But this writer, a better judge than the generality of people, ſeems to me to correct thoſe miſtakes, by ſaying, that they lived —— years before his time, *and no more*. The words *no more*, appear plainly to intimate, as if, in the paſſage in queſtion, Herodotus made the age of the two poets not near ſo great as the common chronologers of his time; whereas his number, as it now

(69) Scholiaſt. on *Thucyd.* p. 64. *Edit. Waſſ.*
(70) Pag. 102.
(71) See Not. 1. 2.

ſtands, differs inconſiderably from what they, moſt probably, made it. What his genuine number was, is difficult to determine; but, from what hath been ſaid, I am inclined to think it was H H H 300; and that it was changed by accident afterwards, by the negligence of tranſcribers, or by ſome interpolator, to make it more conformable to the received chronology, into H H H H 400, as in our preſent printed copies.

And, in favour of this correction, it may be remarked, that Ariſteas, the Proconneſian, as we are informed by Strabo (72), was, according to ſome, Homer's maſter. This Ariſteas ſeems placed, by Herodotus, 340 years before his time (73); but Mr. Dodwell (74) intimates as if he had found, in ſome copies of this author, only 240; and ſays, that this number is confirmed by Tzetzes (75).

I have now finiſhed all that I ſhall trouble your lordſhip with, at preſent, upon this head. What hath been here advanced, your lordſhip will regard,

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(72) Pag. 946.

(73) Τὰ ὃ οἰδα Μεταποντίνοισι τοῖσι ἐν Ιταλίᾳ συγχυρήσαντα μετὰ τὴν ἀφάνισιν τὴν δευτέρην Αριστέω ἔτεσι τετρακοσίοισι καὶ τεσσαρακοσίοισι. Pag. 227.

(74) Quo tamen in loco 240 legunt, ni fallor alia exemplaria, ſuffragante, ut arbitror, in Chiliadibus Joanne Tzetze. *De Cyc. Vet.* p. 130.

(75) After telling the ſame ſtory that Herodotus doth of Ariſteas's death and revival, he adds,

Ἑστὶ τὰ Αεμμάστια λεγόμενα συγγράφει,
Καὶ πάλιν ἀφανίζεται τὸ δεύτερον καὶ θνήσκει.
Καὶ μετὰ διακόσια δὶς εἴκοσι τὰ ἔτη,
Ἐφ' Ηροδότου γέγονε, καὶ πάλιν ἀνεφάνη,
Ὡς φησὶν Ηρόδοτος.——

Hiſt. Chiliad. ii. c. 50.

not as certainty, but probability and conjecture. My deſign hath been to aſcertain, as far as may be, the *true riſe and progreſs of aſtronomy among the antients*, by clearing its hiſtory from *fable* and *mythology*. This hath been the ſubject of ſome former letters to your lordſhip's worthy predeceſſor in the chair: And as the preſent enquiry makes part of the ſame (76) plan, it could be addreſſed to no one ſo properly as to your lordſhip; and, at the ſame time, it gives me an opportunity of expreſſing with what eſteem I am,

My Lord,

Your lordſhip's moſt obedient

and moſt devoted humble ſervant,

October 20, 1753.

G. Coſtard.

LX. *An additional Remark to one of Mr.* William Watſon, *F. R. S. in his Account of the Abbé* Nollet's *Letter concerning Electricity. By* Thomas Birch, *D. D. Secr. R. S.*

Read Jan. 10, 1754.

MR. Watſon, in a note upon his account of the ninth letter of the abbé Nollet concerning electricity, read before this

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(76) See *Letter to* M. Folkes, *Eſq*; *P. R. S.* p. 86.

Society

Society on the 17th of May 1753. takes notice, that as the electrical attraction has been obſerved ſo early, as to be mentioned by Theophraſtus; ſo its luminous appearance, tho' only conſidered as a meteor, is mentioned by Plutarch in his life of Lyſander, Pliny, and other antient as well as ſome modern authors. Seneca particularly affirms, that *Gylyppo Syracuſas petenti viſa eſt ſtella ſuper ipſam lancem conſtitiſſe*: And that in *Romanorum caſtris viſa ſunt ardere pila, ignibus ſcilicet in illa delapſis.* Cæſar, in his hiſtory of the African war, ſays, in a violent ſtormy night, *Legionis pilorum cacumina ſua ſponte arſerunt*: And Livy mentions two ſimilar facts. To theſe I ſhall now add one from Mr. Fynes Moryſon, a writer of unqueſtionable authority, and eminent for his learning, curioſity, and ten years travels through moſt countries of Europe, and part of Aſia. This gentleman, who was ſecretary to Charles Blount, lord Montjoy, lord deputy, and afterwards lord lieutenant, of Ireland, and earl of Devonſhire, of whoſe great ſervices, in the reduction of that kingdom, he has inſerted a very particular hiſtory in his *Itinerary*, obſerves, in that work, Part II. Book ii. p. 176. edit. Lon. 1617. fol. that at the ſiege of Kingſale by the lord deputy Montjoy, where Mr. Moryſon attended him in the camp, on the 23d of December 1601. all the night was clear, with lightning (as in the former nights were great lightnings with thunder), to the aſtoniſhment of many, in reſpect of the ſeaſon of the year. " And I have heard, adds he, by many horſemen of " good credit, and, namely, by captain Pikeman, " cornet to the lord deputy's horſe, a gentleman of " good eſtimation in the army, that this night our

" horſemen

" horſemen ſet to watch, to their ſeeming, did ſee
" lamps burn at the points of their ſtaves, or ſpears,
" in the midſt of theſe lightning flaſhes."

LXI. *Extract of a Letter of the Reverend Mr.* Joſeph Spence, *Profeſſor of Modern Hiſtory in the Univerſity of* Oxford, *to Dr.* Mead, *F. R. S.*

Byfleet near Weybridge, Surrey,
December 7, 1753.

Read Jan. 10, 1754. I Have lately received a letter from Signor Paderni at Portici; in which, ſpeaking of the publication of the antiquities found at Herculaneum, he ſays, *Spero che il primo tomo non tarderà molto tempo ad uſcire*; and then mentions ſome particular things, that had been lately diſcovered among the ruins; a little braſs buſt of ſome unknown philoſopher, of an excellent manner, and perfectly well preſerved: A ſtatue of an orator, in marble; and another braſs buſt, on a term, of a youth, with particularly beautiful hair, and the whole excellent. The artiſt has put his name to the latter, ΑΠΟΛΛΩΝΙΟΣ ΑΡΧΙΟΥ ΑΘΗΝΑΙΟΣ ΕΠΟΙΗΣΕ. He ſays, that the workmen were then juſt entering on ſome nobleman's houſe, as appeared by the rich moſaic pavements, *&c.* and that they were in hopes it would prove a very good new mine.

LXII. *A Letter to the Right Honourable* George *Earl of* Macclesfield, *P. R. S. concerning the Value of an Annuity for Life, and the Probability of Survivorſhip. By Mr.* James Dodſon.

My Lord,

Read Jan. 17, 1754. YOUR Lordſhip's approbation of my eſſay, concerning the method of forming the ſeries for making logarithms, and that for rectifying the circle, without the aſſiſtance of fluxions, has emboldened me to lay before you the following inveſtigations, concerning the value of an annuity for life (ſecured by land), and the probability of ſurvivorſhip, between two perſons of given ages; neither of which have (as I believe) been before attempted to be found, otherwiſe than by fluxions; at leaſt, no ſuch attempt has appeared in public.

I am ſenſible that I ought to apologize for reciting the principles, upon which calculations of this kind are commonly founded, becauſe I know that your Lordſhip is perfectly well acquainted with them; but I plead in excuſe thereof, that I found a great difficulty would attend the rendring my argument intelligible and concluſive, if they were omitted. I am, with the greateſt deference, my Lord,

Your Lordſhip's moſt obliged,

and moſt humble ſervant,

Bell-Dock, Wapping,
Oct. 18, 1753.

J. Dodſon.

The writers upon the ſubject of annuities on lives have very juſtly diſtinguiſhed them into two kinds: In the firſt, the annuitant is entituled to receive a payment, if he be alive on the day on which it becomes due; but if he dies on the preceding day, or ſooner, his heirs have no claim to any part of the payment, ſo to have became due; but in the ſecond, if the annuitant dies at any intermediate time, between the days of payment, his heirs are to receive a part of the annuity, proportional to the time elapſed, between the preceding day of payment and the annuitant's deceaſe.

This latter kind of annuities have been diſtinguiſhed from the former, by the words, *ſecured by a grant of lands*; becauſe, where lands are leaſed for lives, the conditions are generally ſuch as are above deſcribed.

The values of the firſt kind of annuities have been inveſtigated upon principles purely arithmetical; but, in order to perform the latter, fluxions have been uſed (as I humbly conceive) without any neceſſity: But as the inveſtigation of the former may be uſefully made a part of the latter, I ſhall firſt recite the method of performing that, and then proceed to attempt the other, upon the ſame principles.

If, with the ſagacious mathematician Mr. Abraham De Moivre, we ſuppoſe the decrements of life to be equal (*viz.* that out of a number of perſons, alive at a given age, equal to the number of years that a perſon of that age hath a poſſibility of living, there will die one in each year, till they are all extinct); then, out of a number of chances equal to that number of perſons, which may, for inſtance,

be

be 36, all but one are favourable, in the firſt year, to any individual; and, conſequently, it is 35 to 1, that he receives one payment of the annuity, by living till it becomes due; that is, the probability of his receiving it, is $\frac{35}{36}$, and that of his not receiving it, $\frac{1}{36}$.

Again; ſince, by ſuppoſition, there dies but one perſon in the firſt year, and one in the ſecond; there are but two chances, in the 36, againſt his receiving the ſecond payment, by living till it becomes due; and, conſequently, $\frac{34}{36}$ will be the probability of his receiving that alſo; the probability of his dying in that year being $\frac{1}{36}$, as before.

In like manner it may be proved, that the probability of his receiving the third, fourth, fifth, &c. payment, will be $\frac{33}{36}$, $\frac{32}{36}$, $\frac{31}{36}$, &c. and therefore, if the annual payments were each 1 *l.* and if the intereſt of money was not to be conſidered, we might conceive theſe ſeveral probabilities, as the preſent worths of the ſeveral payments, and the ſum of them would be the value of an annuity of the firſt kind.

But ſince the intereſt of money neceſſarily enters the proceſs, and ſince the payments become due at the end of the firſt, ſecond, third, &c. year; therefore the firſt of theſe payments muſt be diſcounted for one year, the ſecond for two years, the third for three years, &c. and the ſum of their preſent worths will be the value of an annuity of the firſt kind, to continue during the life of a perſon, who may poſſibly live 36 years; and this ſum may be found by an eaſy and well-known proceſs (from the common tables of compound intereſt and annuities), which need not be inſerted here.

The annuity ſecured by land muſt neceſſarily be of greater value than the above; becauſe, altho' the annuitant dies before the payment becomes due, yet his heirs are to receive a part thereof; the annuitant, therefore, in this caſe, hath not only the probability $\frac{35}{36}$ of receiving the firſt payment, but he hath alſo an expectation upon part of the probability $\frac{1}{36}$, which, in the firſt caſe, was wholly againſt him. Now it may be eſteemed an equal chance (ſuppoſing him to die in the firſt year) whether that deceaſe happens before the expiration of half that year, or after it; and if it happens before, he is to receive leſs than half the annual payment; but if after it, more.

The annuitant may, therefore, be ſuppoſed to have an equal chance, if he fails of receiving the whole firſt payment, yet of receiving half thereof; and, conſequently, half of the probability, $\frac{1}{36}$, which was before totally againſt him, will (in this caſe) be favourable to him; and his expectation of receiving either the whole, or at leaſt half of the firſt payment, will be $\frac{35}{36} + \frac{1}{72}$.

In like manner, ſince the probability of his dying, in the ſecond year, is, alſo, $\frac{1}{36}$; we may (by arguing in the ſame manner) prove, that one half thereof will (in this caſe) become favourable to him; and, conſequently, that $\frac{34}{36} + \frac{1}{72}$ will be the probability of his receiving the whole, or at leaſt half, of the ſecond payment.

It appears, therefore, that, for every year which he hath the poſſibility of living, he will (in this caſe) have the probability ($\frac{1}{72}$ or) half of $\frac{1}{36}$ in his favour, more than he had in the former caſe; and therefore, if the preſent worths of the conſtant ſum $\frac{1}{72}$ *l.* (ſuppoſed

poſed to be due at the end of one, two, three, &c. years) be found, and added to the value of the annuity, according to the former caſe, the ſum will be the value of an annuity, ſecured by land, to continue during the life of a perſon who may poſſibly live 36 years.

Now ſince the ſums, whereby the former annuity is to be increaſed, conſiſt of the preſent worths of that fraction of a pound ſterling (whoſe numerator is unity, and denominator twice the number of years that the annuitant can poſſibly live) ſuppoſed to be due at the end of each of one, two, three, &c. years; it will follow, that their amount, or the difference between the values of the two annuities, will be equal to the quotient, found by dividing the value of an annuity of 1 *l.* certain, for as many years as the annuitant can poſſibly live, by twice that number of years: And, therefore, if to the value of an annuity for life, of the firſt kind, we add the quotient ſo found; the ſum will be the value of an annuity, of the ſecond kind, for the ſame life.

When I had thus inveſtigated the value of this annuity, I compared the reſult with that Mr. De Moivre has deduced from fluxions, which is publiſhed in N° 473. of the *Philoſophical Tranſactions*; and found, that they agree to more than a ſufficient exactneſs, for computations of this nature: I have, therefore, annexed that compariſon hereto.

The above-mentioned author, and others, have alſo calculated the probabilities of ſurvivorſhip, by a fluxional proceſs; which probabilities will flow from what is above premiſed, by a very eaſy calculus; in the doing which, the above yearly probabilities of the an-

nuitant's receiving the whole firſt, ſecond, third, &c. payment, or at leaſt the halves of them (being, in fact, the probabilities of his living the whole of thoſe years, or at leaſt the halves of them), are called the expectations of life, in thoſe years.

Mr. De Moivre has proved, on another occaſion, that, if there be an expectation, to take place upon the happening of two independent events, the probability of the compound event, will be the product of the two ſingle probabilities: Now, if there be an expectation, depending on one man's ſurviving another, it muſt neceſſarily be compounded of the probabilities of the one's living, and of the other's dying. Now let the perſon, upon whoſe ſurviving the expectation depends, be of ſuch an age, that he may poſſibly live 36 years; and let the perſon, who is to be ſurvived, be of that age, that he may poſſibly live 43 years: Then ſince the ſurvivor's expectation of life will, in the firſt, ſecond, third, &c. year, be $\frac{35}{36} + \frac{1}{72}$, $\frac{34}{36} + \frac{1}{72}$, $\frac{33}{36} + \frac{1}{72}$, &c. and ſince the probability of the other's dying, in any one year, will be $\frac{1}{43}$; therefore, if the former be ſeverally multiplied by the latter, the products ariſing, *viz.* $\overline{\frac{35}{36} + \frac{1}{72}} \times \frac{1}{43}$, $\overline{\frac{34}{36} + \frac{1}{72}} \times \frac{1}{43}$, $\overline{\frac{33}{36} + \frac{1}{72}} \times \frac{1}{43}$, &c. will exhibit the probabilities of the ſurvivorſhip's taking place, in the firſt, ſecond, third, &c. years.

For the above expectations contain the probabilities of the ſurvivor's living the whole, or (at leaſt) the halves of the firſt, ſecond, third, &c. years; and the conſtant factor, $\frac{1}{43}$, is the probability of the other's dying, ſome time within one of thoſe years; which may, therefore, be either in the firſt, or in the ſecond, half year, of either of them; thro' both of which periods,

periods, the probability of the ſurvivor's living is above exhibited; and, conſequently, the probabilities of the ſurvivorſhip's taking place, in thoſe times, will be ſeverally repreſented by the above products.

Now the ſum of 36 terms of this ſeries of products, will, upon computation, appear to be $\frac{1}{4}\frac{8}{3}$; which is, therefore, the probability of the ſurvivorſhip required.

But if, inſtead of requiring, as above, the probability of the elder perſon's ſurviving the younger, it were required to find the probability of the younger perſon's ſurviving the elder; then, ſince it is almoſt a certainty, that both of them will not die in the ſame moment of time; we may, by denoting that certainty by unity (agreeable to another principle eſtabliſhed in the doctrine of chances) determine the probability required to be ($1 - \frac{1}{4}\frac{8}{3}$ or) $\frac{2}{4}\frac{5}{3}$: The algebraic inveſtigation of each of theſe caſes is annexed, to which I beg leave to refer, for a farther illuſtration; and ſhall only obſerve, farther, that the above two reſults will, upon compariſon with thoſe given, upon a fluxional proceſs by Mr. De Moivre, in his treatiſe of annuities on lives, appear to coincide with them exactly.

The probability of any order of ſurvivorſhip, that can happen among three perſons, and, conſequently, that of one perſon's ſurviving two others, may, likewiſe, be inveſtigated upon ſimilar principles, without the aſſiſtance of fluxions; but as this problem admits of ſix caſes, and the algebraic proceſs is of a length, too great for the deſigned limits of this eſſay, I beg leave to poſtpone it.

A com-

A comparison of the result of the beforegoing investigation of the value of an annuity for life (secured by land), with the value thereof, given by Mr. De Moivre, *in p.* 67. *of* N° 473. *of the* Philosophical Transactions.

If the annual payment, or rent, be supposed to be 1 *l.*; if the rate of interest (that is, the amount of 1 *l.* and its interest for one year) be denoted by r; the number of years which the annuitant may possibly live (called by Mr. De Moivre the complement of life) by n; and the value of an annuity of 1 *l. per annum*, certain for n years, by P: Then, by *Prob.* I. of Mr. De Moivre's treatise of annuities on lives, the value of an annuity (not secured by land) for the life of a person, whose complement is n, will be $\frac{1 - \frac{r}{n} P}{r - 1}$; to which if $\frac{P}{2n}$ be added, the sum $\frac{1 - \frac{r}{n} P}{r - 1} + \frac{P}{2n}$ will, according to the result of the above investigation, be the value of an annuity (secured by land) for the life of the same person: And if the hyperbolic logarithm of r be denoted by α; then, by the above-quoted *Philosophical Transaction*, $\frac{1}{r - 1} - \frac{P}{\alpha n}$ will be the value of the same annuity, according to Mr. De Moivre: Let us proceed, therefore, to examine, whether

ther theſe two expreſſions, *viz.* $\frac{1-\frac{r}{n}P}{r-1}+\frac{P}{2n}$ and $\frac{1}{r-1}-\frac{P}{\alpha n}$ are equal, or nearly ſo.

It is well known, that the hyperbolic logarithm of any number, r, is equal to the infinite ſeries, $\frac{2}{1}\times\frac{r-1}{r+1}+\frac{2}{3}\times\overline{\frac{r-1}{r+1}}\Big|^{3}+\frac{2}{5}\times\overline{\frac{r-1}{r+1}}\Big|^{5}$ &c. and, in this caſe (ſince the intereſt of money has not, for many years, exceeded 5 *l. per cent.* and is continually decreaſing), r will be always expounded by ſome of the following numbers, 1,05. 1,04. 1,03. &c. or others intermediate to them; in the greateſt of which (*viz.* 1,05) $r-1$ will be expounded by (,05 or) $\frac{1}{20}$, and $r+1$ by (2,05 or) $\frac{41}{20}$; whence, $\frac{2}{1}\times\frac{r-1}{r+1}$, the firſt term of the ſeries will be $\frac{2}{41}$, and $\frac{2}{3}\times\overline{\frac{r-1}{r+1}}\Big|^{3}$ will be $\left(\frac{2}{3}\times\frac{1}{=41^{3}}=\right)\frac{2}{206763}$, a fraction too ſmall to affect a calculation of this kind; and therefore, $\frac{2}{1}\times\frac{r-1}{r+1}$, the firſt term of the ſeries, will be nearly equal to α, and may be wrote for it.

Now by writing $\frac{2}{1}\times\frac{r-1}{r+1}$ for α, the value of the expreſſion, $\frac{1}{r-1}-\frac{P}{\alpha n}$, will become $\frac{1}{r-1}-\frac{1}{r+1}$

$\frac{\overline{r+1} \times P}{\overline{r-1} \times 2n}$; and it remains, only, to prove the equality of the expreſſions, $\frac{1-\frac{r}{n}P}{r-1} + \frac{P}{2n}$ and $\frac{1}{r-1} - \frac{\overline{r+1} \times P}{\overline{r-1} \times 2n}$, which may be done as follows:

Put $\frac{1-\frac{r}{n}P}{r-1} + \frac{P}{2n} = N.$

Then $1 - \frac{r}{n}P + \frac{r-1}{2n}P = \overline{r-1} \times N;$

But, becauſe $\frac{r}{n} = \frac{2r}{2n},$

And $\frac{2r}{2n} - \frac{r-1}{2n} = \frac{r+1}{2n};$

Therefore $1 - \frac{r+1}{2n}P = \overline{r-1} \times N,$

Whence $\frac{1}{r-1} - \frac{\overline{r+1} \times P}{\overline{r-1} \times 2n} = N:$

And the equality of the two expreſſions is evident: From whence it appears, that the value of an annuity for life (ſecured by land) might have been found, exactly enough, without the aſſiſtance of fluxions.

The

The inveſtigation of the probabilities of ſurvivorſhip between two perſons of given ages.

Let the complement of the younger life be denoted by n, and that of the elder by p: Then,

Firſt, If the probability of the elder perſon's ſurviving the younger be required, let the ſymbol p be uſed inſtead of the number 36, and it will (from the argument above uſed) appear, that the expectations of the ſurvivor's life, for the firſt, ſecond, third, &c. years, will be repreſented by $\left(\frac{p-1}{p} + \frac{1}{2p}, \frac{p-2}{p} + \frac{1}{2p}, \frac{p-3}{p} + \frac{1}{2p}, \&c.\right.$ or $\left.\frac{2p-1}{2p}, \frac{2p-3}{2p}, \frac{2p-5}{2p}, \&c.\right)$ which expectations, being ſeverally multiplied into $\frac{1}{n}$, the yearly probability of the other's dying, will give $\frac{2p-1}{2pn}, \frac{2p-3}{2pn}, \frac{2p-5}{2pn}, \&c.$ for the probability of the ſurvivorſhip's taking place, in the firſt, ſecond, third, &c. years.

Now ſince, by the hypotheſis, the ſurvivor cannot poſſibly out-live p years; therefore, only, p terms of the above ſeries are to be uſed; which ſeries, being an arithmetical progreſſion, whoſe greateſt term is $\frac{2p-1}{2pn}$, leaſt term $\frac{1}{2pn}$, and number of terms p; the ſum thereof will, by a well-known rule, be $\left(\frac{2p-1}{2pn} + \frac{1}{2pn} \times \frac{p}{2} = \frac{2p}{2np} \times \frac{p}{2} =\right) \frac{p}{2n}$; which

expreſſion is exactly the ſame as Mr. De Moivre gives, in the 44th page of the 3d edition of his treatiſe of annuities on lives; which by the demonſtration thereof, given in fol, 116. of the ſame work, appears to have been obtained by a fluxional proceſs.

Secondly, If the probability of the younger perſon's ſurviving the elder be required; then, if $\frac{2n-1}{2n}$, $\frac{2n-3}{2n}$, $\frac{2n-5}{2n}$, &c. the yearly expectations of the continuance of the life of the younger, be ſeverally multiplied by $\frac{1}{p}$, the yearly probability of the elder's dying; the products, $\frac{2n-1}{2np}$, $\frac{2n-3}{2np}$, $\frac{2n-5}{2np}$, &c. will give the probabilities of the ſurvivorſhip's taking place, in the firſt, ſecond, third, &c. years.

And here, as before, the ſurvivorſhip muſt neceſſarily take place, before, or at, the end of p years, the longeſt time that the elder can poſſibly live, by ſuppoſition; and therefore, the ſum of p terms, of the ſeries above given, will be the anſwer.

Now this ſeries is, alſo, an arithmetical progreſſion, whoſe greateſt term is $\frac{2n-1}{2np}$; leaſt, $\frac{2n-2p+1}{2np}$; and number of terms, p; whence $\left(\frac{2n-1}{2np}+\frac{2n-2p+1}{2np}\right)\times\frac{p}{2}=\frac{4n-2p}{2np}\times\frac{p}{2}=\frac{4n-2p}{2n}\times\frac{1}{2}=\frac{2n-p}{2n}=\Big)1-\frac{p}{2n}$ will be the probability required; to which, if $\frac{p}{2n}$, the pro-


J. Meynde Fecit.

los. Trans. Vol. XLVIII. TAB. XV. *p.* 499.



d. Meynde Fecit.

probability of the elder's ſurviving the younger, be added; the ſum will be unity, as was above obſerved.

LXIII. *A Letter to Mr.* Peter Collinſon, *F. R. S. concerning the Pheaſant of* Penſylvania, *and the* Otis Minor. *By Mr.* George Edwards.

To Mr. Peter Collinſon, *F. R. S.*

College of Phyſicians,
Jan. 10, 1754.

SIR,

Read Jan. 17, 1754.

ACcording to your requeſt, and by your aſſiſtance, I have drawn up a brief account of the fowl, called a pheaſant in Penſylvania, in order to lay it, together with the birds, before the *Royal Society*.

The coloured print, *Plate* XV. repreſents what is called the pheaſant in Penſylvania and other provinces of North America, tho' it rather belongs to that genus of birds, which in England we call heathcocks, moor-game, or grous. It is near as big as a pheaſant, of a browniſh colour on the head and upper ſide, and white on the breaſt and belly; beautifully variegated with lighter and darker colours on the back, and ſpots of black on the under ſide. Its legs are feathered down to the feet, which will appear by the bird preſerved dry, here preſent, as well as by the print in miniature. As this bird is, in my judgment, wholly unknown to the curious of our country, I

ſhall, by foreign helps, give the beſt account of it that I can gather. It was ſent to England, a year or two ago, by Mr. John Bartram to you, with a letter giving ſome account of it, and other matters; out of which letter I have extracted what follows: 'Our pheaſant was, I believe, wholly un-
'known to Cateſby, it being more northern than Ca-
'rolina. They have been common (in Penſylvania),
'but now moſt of them are deſtroyed in the lower
'ſettlements, tho' the back Indian inhabitants bring
'them to market. When living, they erect their
'tails like turkey-cocks, and raiſe a ring of feathers
'round their necks, and walk very ſtately, making a
'noiſe a little like a turkey, when the hunter muſt
'fire. They thump in a very remarkable manner,
'by clapping their wings againſt their ſides, as is ſup-
'poſed, ſtanding on a fallen tree. They begin their
'ſtrokes at about two ſeconds of time diſtant from
'each other, and repeat them quicker and quicker,
'until they ſound like thunder at a diſtance, which
'laſts about a minute, then ceaſes for 6 or 8 minutes,
'and begins again. They may be heard near half a
'mile, by which the hunters find them. They ex-
'erciſe their thumping in a morning and evening in
'the ſpring and fall of the year. Their food is ber-
'ries and ſeeds. Their fleſh is white, and good. I
'believe they breed but once a year in the ſpring,
'and hatch 12 or 14 at a ſitting; and theſe keep to-
'gether till the following ſpring. They cannot be
'made tame. Many have, to their diſappointment,
'attempted it by raiſing them under hens; but, as
'ſoon as hatched, they eſcaped into the woods,
'where they either provided for themſelves, or pe-
riſhed.'

In

In order to be farther informed of this bird, I wrote to Mr. Brooke, ſurgeon of Maryland, who was lately in London, to aſſiſt me with what he knew of the nature of it; who obliged me with a letter, wherein he ſays, that the print I ſent him (which was the ſame with this here preſent) repreſented their pheaſant juſt as it appears when ſurpriſed, and going to take wing. 'They breed in all parts of 'Maryland, except near the eaſtern ſhores. They 'lay their eggs in neſts made of dry leaves by the 'ſide of a fallen tree, or at the root of a ſtanding 'one: They lay from 12 to 16 eggs, and hatch in 'the ſpring; but how long they ſit, I cannot tell. I 'have found their neſts, when I was a boy, and have 'endeavoured to take the old one, but never could: 'She would let me put my hand almoſt on her be-'fore ſhe quitted her neſt; then ſhe would flutter 'juſt before me for 100 yards, or more, to draw me 'off from her neſt, which could not afterwards be 'eaſily found. The young ones leave the neſt as ſoon 'as hatched, and, I believe, live at firſt on ants and 'worms: When they are a few days old, they hide 'themſelves amongſt the leaves, that it is hard to find 'them. When they are grown up, they feed on the 'berries, fruits, and grain, of the country. Tho' 'the pheaſant hatches many young at a ſitting, and 'often ſits twice a year, the great number and va-'riety of hawks amongſt us, feeding on them, pre-'vents their increaſing faſt. The beating of the 'pheaſant, as we term it, is a noiſe chiefly made in 'the ſpring by the cock birds. It may be diſtinctly 'heard a mile in calm weather. They ſwell their 'breaſts like a pouting pigeon, and beat with their

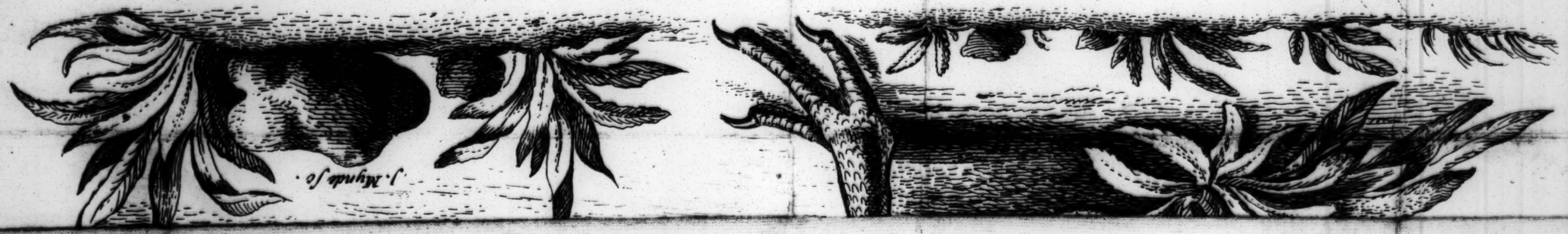
'wings,

'wings, which ſounds not unlike a drum. They 'ſhorten each ſound in ſtroke, till they run into one 'another undiſtinguiſhed.'

Lahontan, in his voyage to North America, Vol I. p. 67. ſpeaking of the fowls about the lakes of Canada, I believe, mentions this ſame pheaſant as follows:

'Their flapping makes a noiſe like a drum, all 'about, for the ſpace of a minute; then the noiſe 'ceaſes for half a quarter of an hour, after which it 'begins again. By this noiſe we were directed to the 'place, where the unfortunate moor-hens ſat, and 'found them upon rotten moſſy trees. By flapping 'one wing againſt the other, they mean to call their 'mates; and the humming noiſe, that enſues there-'upon, may be heard a quarter of a league off. This 'they do only in the months of April, May, Sep-'tember, and October; and, which is very remark-'able, a moor-hen never flaps in this manner but upon 'one tree. It begins at break of day, and gives over 'at 9 in the morning, beginning again an hour before 'ſun-ſet, and flaps its wings till night.' This is all the light I could gather, relating to the pheaſant of North America.

The coloured print, *Plate* XVI. repreſents the otis minor, anas campeſtris, canne petiere, the field duck, which was taken in the weſt of England, and laid before the *Royal Society* about three years ago: And as there happened to be no gentleman preſent, who knew the bird, Mr. Haukſbee, by order of the ſociety, ſent the porter with it to me, to deſire I would communicate to the ſociety what I knew of it. I, ac-


J. Mynde fc.

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according to the Society's requeſt, gave in what account of it I could collect from ornithologiſts, having never ſeen the bird till then; which account was favoured with a reading before the Society. I find figures of it in the following authors on animals; *viz.* Bellon, Geſner, Aldrovand, Willoughby, Johnſon, and others; but as their figures are ſmall, and the later authors have only copied the more antient, and none of them are drawn with the correctneſs one would wiſh, none of them expreſſing the lighter, darker, and ſpotted parts, which are remarkable, and add to the beauty of a draught; I have eſſayed to make a more perfect figure than has hitherto appeared in print of this bird. I have done it on a larger ſcale than what has gone before it, which has given me the better opportunity of expreſſing its different ſhades and ſpots, which are blended throughout its feathering, which could not have been done on a ſmaller ſcale. The deſcriptions of this bird, under the various names here given, with little variation, may be found, with the figures, in the ſeveral authors abovementioned. I think the modern authors agree, that this bird was unknown to the antients. I am,

SIR,

With great reſpect,

Your very humble ſervant,

George Edwards.

LXIV. *A Letter to Mr.* Peter Collinſon, *F. R. S. concerning a particular ſpecies of Coralline. By Mr.* John Ellis, *F. R. S.*

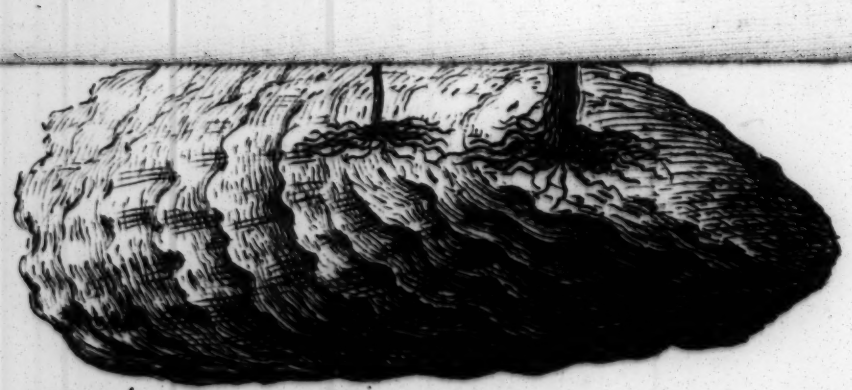
SIR,

Read Febr. 7, 1754.

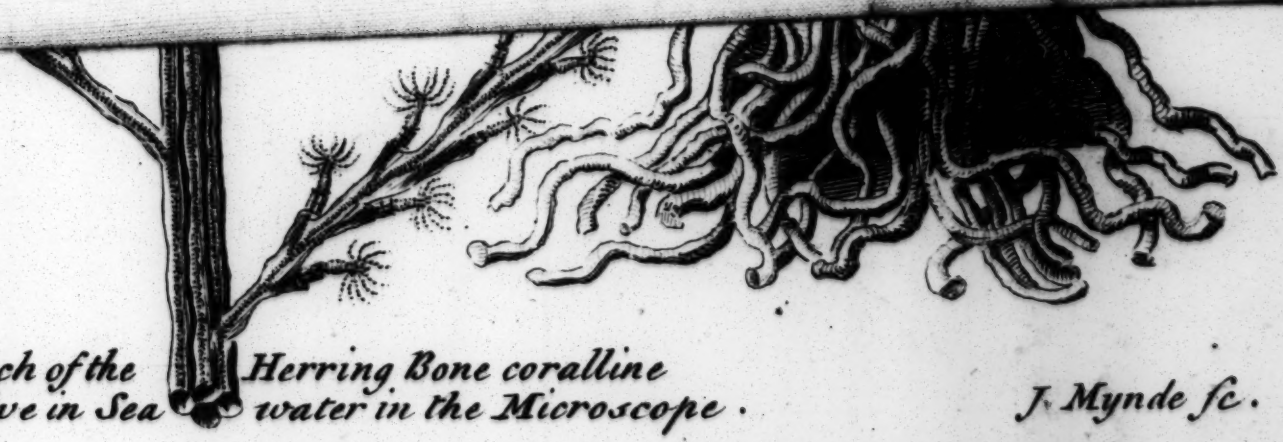
AMong the obſervations I have lately made on the marine productions, I find, that many corallines, as well as corals, are compoſed of a great number of tubes, which proceed from animals; and as theſe tubes are made of different materials in different ſpecies, ſo are they diſpoſed in variety of different forms. Some are united cloſely and compactly together, as in the red coral, ſee *Plate* XVII. letter *A*; and in ſome ſpecies of the white, as at letter *B*; in both of which they appear, combined together, forming irregular ramifications, like trees: Others riſe in tufts, like groupes of the tubular ſtalks of plants, diſtinct from one another. Two ſorts of theſe the fiſhermen frequently take up at ſea in their nets, particularly near the Buoy of the Nore, at the opening of the river Thames. When theſe are firſt taken out of the ſea, and immediately put into a baſon of ſea water, you may obſerve, that each tube has its proper polype, ſitting on it, of a moſt beautiful crimſon colour. Letter *D*, in *Plate* XVII. gives us the figure of the largeſt kind, called, in Ray's *Synopſis*, Ed. 3. p. 31. *Adianti aurei minimi facie planta marina*; and letter *C* is a ſmaller kind, called, in Ray's *Synopſis*, Ed. 3. p. 39. *Fucus Dealenſis fiſtuloſus laringæ ſimilis.*

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Herring Bone Coralline on an Oyster Shell.



A small branch of the Herring Bone coralline as it appear'd alive in Sea water in the Microscope.

J. Mynde sc.

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opening of the Tubes in
the Inside and at the ends
of the branches.

C

Tubular Coralline
wrinkled like the
wind pipe.

Tubular Coralline
like Oaten pipe.

E

G

F

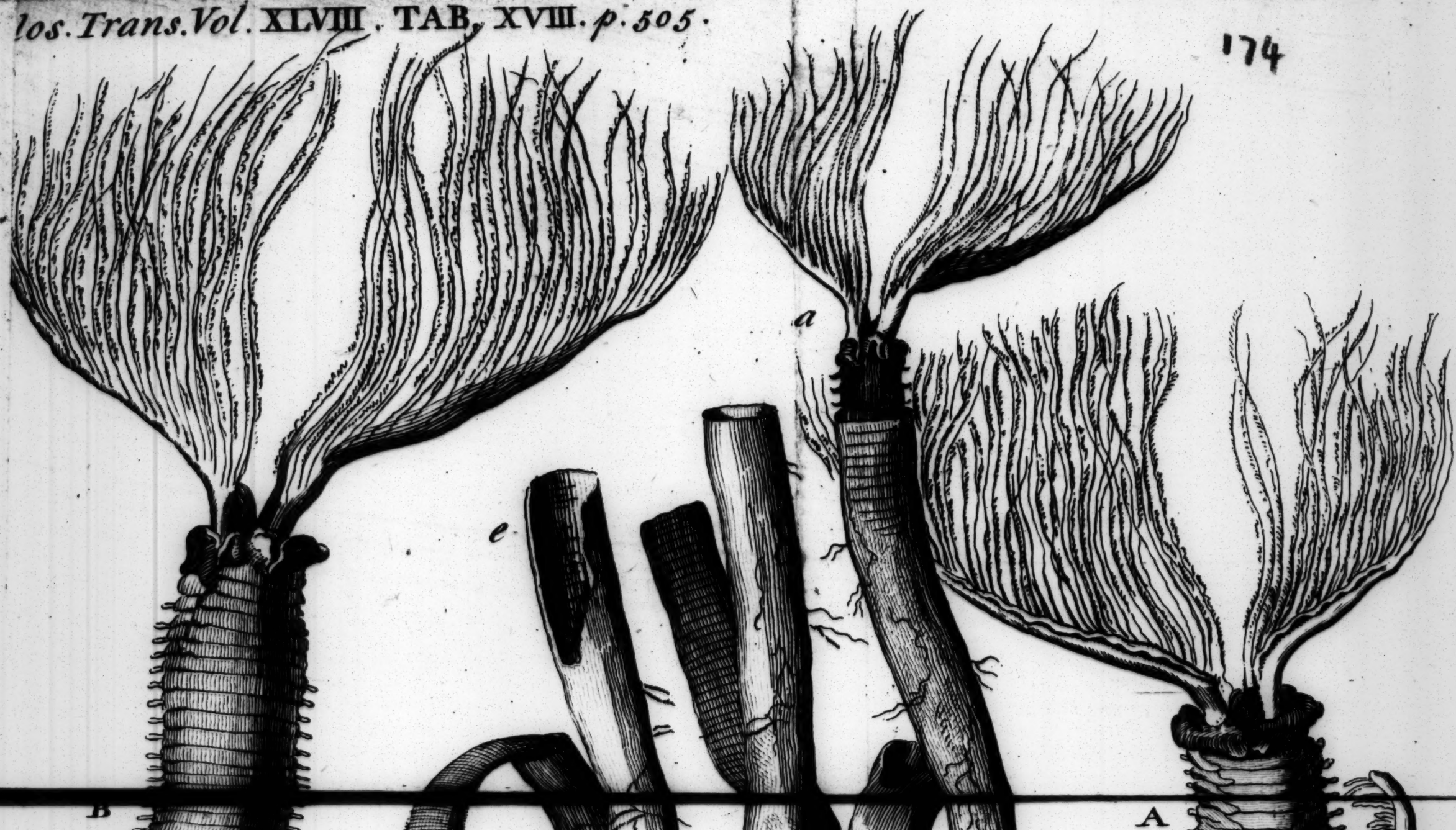
The Root of the
Herring Bone
Coralline magni-
fied being a
combination of
Vermicular tubes

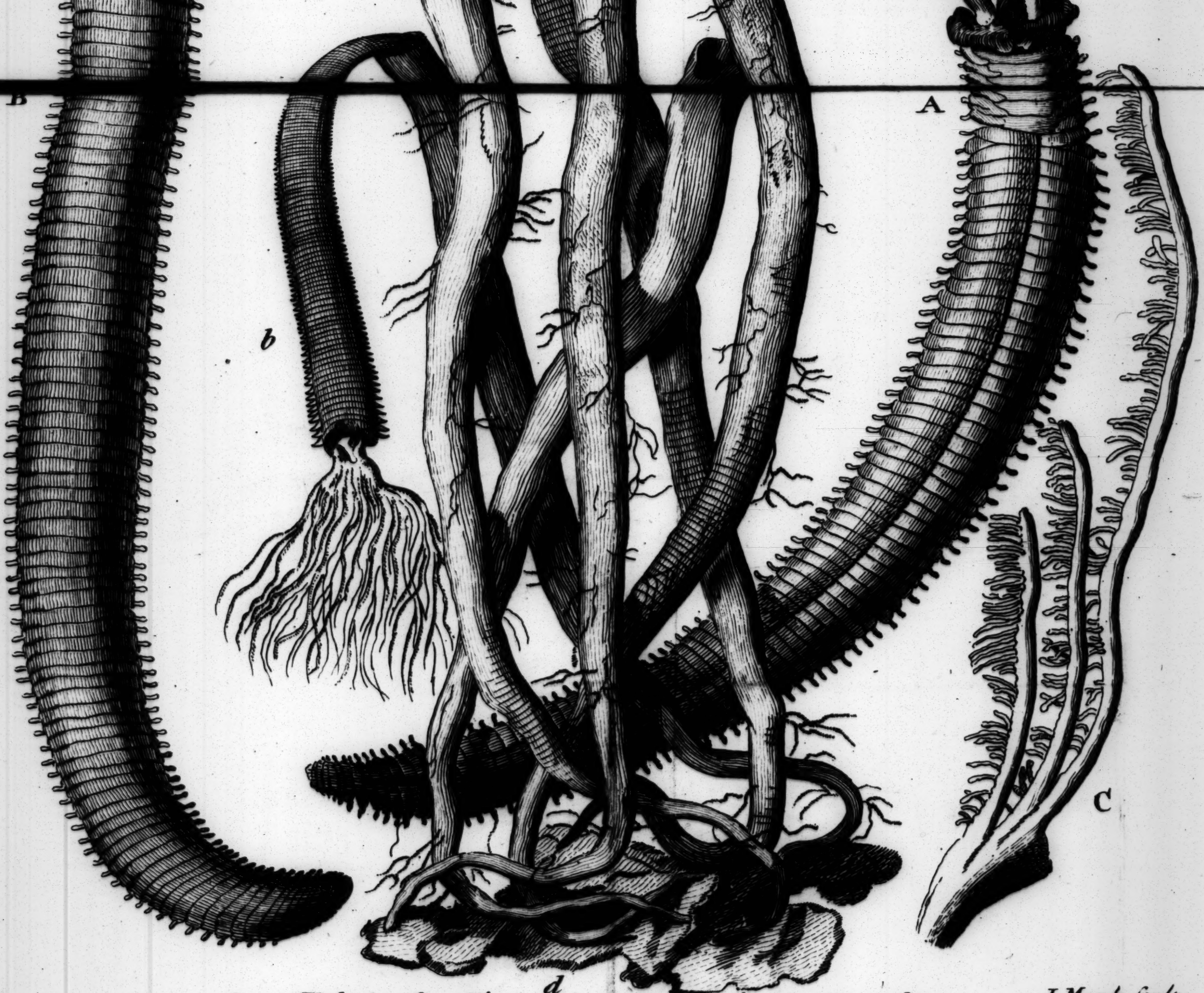
Herring Bone Coralline on an Oyster Shell.

A small branch of the Herring Bone coralline
as it appear'd alive in Sea water in the Microscope.

J. Mynde sc.

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Great Tubular Coralline from Malta with it's Scolopendras.

J. Mynde sculp.

To theſe I ſhall join thoſe curious tubes, with their animals, which you were ſo kind to ſend me; and think this ſpecies may not improperly be called *Corallina tubularia Melitenſis, cum ſcolopendris ſuis, tentaculis duobus duplicato-pinnatis, inſtructis.*

Upon taking the tubes and animals of this curious Malteſe coralline out of the ſpirits of wine, wherein they had been preſerved, I perceived a ſmall ſlimy bag, in which they ſeemed to be inſerted, and to take their riſe from, as may be obſerved at letter *d*, *Plate* XVIII. What has been the uſe of this bag, is uncertain, unleſs we conjecture it to have been the matrix of ſeveral of theſe ſcolopendras in their embrio ſtate.

The tubes, which are built by the incloſed animals, as they riſe in height, gently increaſe in diameter: The texture of their outſide coat is formed of an aſh-coloured earthy matter, of different ſhades in different ſtrata, and cloſely united to an inner coat, which is of a tough, horny, tranſparent, and very ſmooth ſubſtance; the cavity, or inſide, of the tube, is perfectly round, tho' the animal is of a long compreſſed figure, like a leach extended. It appears, from the marks of its feet on the inſide of the tube, that it can turn itſelf freely about, and move up and down, the better to attack and ſecure its prey.

This ſcolopendra has two very curious and remarkable tentaculi, or arms, the left much larger than the right; theſe are doubly feathered, as may be ſeen, in the magnified part, at *C*: The number of feet on each ſide of the body of this animal exceeds one hundred and fifty.

The annexed *Plate* XVIII. will beſt explain the reſt; where *b* is the belly-part of the animal, in its natural ſize, hanging out of its tube.

B is the ſame ſide of the whole animal a little magnified.

a is the back-part of the head of the animal, ſitting in its tube.

A is the back-part of the whole animal a little magnified.

e ſhews the inſide of the tube with the ſtrata, or rings, ſeen through the horny inner coat.

I return you my thanks for this favour, and think, that the largeneſs and diſtinctneſs of the animals and tubes, in this ſpecies, ſerve greatly to illuſtrate that genus of corallines which I have called tubulary. I am,

S I R,

Your much obliged, and

obedient humble ſervant,

Lawrence Lane,
Feb. 7, 1754.

John Ellis.

P. S. The coralline called in Ray's *Synopſ.* Ed. 2. p. 2. and Ed. 3. p. 36. N° 15. *Fruticulus marinus, cauliculis craſſiuſculis teretibus rigidis, pennatus*, which I have named the herring-bone coralline, and which is very common on oyſters all the winter ſeaſon, ſhews remarkably, by the help of a common magnifying glaſs, the tubulary

lary ſtructure, not only of ſome of the corals and corallines, but of the keratophytons, or ſea feather; only with this difference, that the tubes of the herring-bone coralline are of a ſpongy elaſtic nature, and always remain open; whereas the others, being of a more ſoft and viſcid nature, by time, and the heat of the climate, are compreſſed together, and harden, ſome into ſtone, and ſome into horn or wood: But this I may explain, perhaps, more clearly hereafter.

In *Plate* XVII. you have, at *E*, the natural ſize and appearance of the herring-bone coralline; at *F* and *G* the root, and one of the upper branches, are magnified, to ſhew the tubes.

LXV. *Obſervations on the late ſevere cold Weather.* *By* William Arderon, *F. R. S.* *and* Henry Miles, *D. D. F. R. S.*

Part of a Letter from William Arderon, *F. R. S. to* Henry Baker, *F. R. S. containing Obſervations, made at* Norwich, *on the late ſevere cold Weather.*

Dear Sir,

Read Feb. 27, 1754. I Have not time to ſend you now a diſtinct account of all the obſervations I have been making this ſevere ſeaſon; but the few that follow will, I hope, excuſe me to you, for the preſent. They were taken by thermometers expoſed to

the open air, in my garden, which varied ſometimes 40 or 50 degrees in 24 hours; the cold coming, as it were, by fits, in a ſurpriſing and unuſual manner.

December 30, at 11 o'clock at night.—All the ſpirits in Haukſbee's thermometer retired into the ball, and Farenheit's ſtood at 20 degrees: At this time I let down a Farenheit's thermometer into the river, to the depth of 4 feet, during 12 minutes; and, when taken up, it ſtood at 33 degrees.

This ſame evening I expoſed an open glaſs jar full of water, in my garden, to be frozen; and in the morning it was all ſolid ice, riſing in the middle, in figure like the fruſtum of a cone. I expoſed alſo, in the ſame place, an open glaſs of ale, which froze even to the bottom, in a very odd manner; for the watry or weaker parts were frozen into plates of ice, ſticking one to another by their edges, the more ſpirituous parts remaining between them, in their interſtices, unfrozen; which being drained off into another glaſs, the taſte was almoſt as ſtrong as brandy, with an high flavour of the hop.

The rhine, or hoar-froſt, now obſerved on horizontal planes, was compoſed of curious thin figured plates, ſtanding perpendicular thereto; ſome, at leaſt, four-tenths of an inch in length.

Dec. 31.—This evening the cold was the moſt intenſe I have obſerved it this ſeaſon; for at ten o'clock Farenheit's thermometer ſtood at 15 degrees.

January 1.—This afternoon it began to thaw, and in the night froze again, whereby, next morning, our buildings in general appeared as if they had been white-waſhed on the outſide, being caſed all over with

rhine

rhine and ice; and the inſides of garrets and out-houſes were covered in the ſame manner.

Jan. 31.—I expoſed a glaſs of proof ſpirits, impregnated with the eſſence or oil extracted from the peel of oranges, at 10 in the evening, in my garden, when Haukſbee's thermometer ſtood at 93°: At 8 next morning, I found it no way affected by the froſt; nor did there ſeem any difference either in taſte, or ſmell.

February 6.—This morning, at 8 o'clock, I expoſed, in my garden, a drinking-glaſs of water, which was completely frozen over in one minute's time; and in 15 minutes the ice was above one-tenth of an inch in thickneſs. Farenheit's thermometer then ſtood at 21 degrees.

A coarſe grey thread, two feet in length, being dipped in water, froze, in 4 ſeconds, ſo ſtiff, that I took it by one end, and held it upright, as if it had been a piece of wire.

If any part of the human ſkin, the finger, for inſtance, was wet with ſpittle, and immediately preſſed on a piece of iron, in the open air, it would be froze ſo faſt, as to ſtick to it; and, if plucked away haſtily, would endanger the tearing off the ſkin from the fleſh. I tried the ſame experiment upon lead; but the ſticking was much leſs, and to wood the finger did not ſtick at all.

In ſome places the ice was one-eighth of an inch thick, for ſeveral days together, within-ſide of our windows (and that even in rooms where fire was kept); and when the weather grew warmer, it did not fall in drops, but vaniſhed imperceptibly into the air, by which it had been brought thither.

Theſe

Theſe plates or caſes of ice were ſometimes an aſſemblage of an infinite number of particles not much unlike the ſcales of fiſhes: Sometimes they reſembled ſmall ſpines, or the cryſtal ſhootings of various kinds of ſalts; and ſometimes they repreſented a variety of landſcapes with trees and plants, from one to three or four inches in length, in ſo beautiful and admirable a manner, as neither pen nor pencil can expreſs.

Our ſtreets were now entirely covered with congealed ſnow and ice, which being very much trampled on by men and beaſts, as well as ground by carriages, a great part thereof was reduced to a kind of powder, like duſt in the ſummer-time, and would probably have been blown about, had there been a ſtrong wind to raiſe it.

I would not trouble you with the whole train of my obſervations on the late froſt, but have ſelected what I thought moſt remarkable, which, I doubt not, you will candidly accept, from, dear Sir,

Your moſt humble ſervant,

Norwich, Feb. 11, 1754.

William Arderon.

Mr. Baker having received this morning, from the Reverend Dr. Miles, F. R. S. ſome farther obſervations on the late cold weather, begs leave to add them in his own words.

Part of a Letter from Henry Miles, *D. D.*

Pray, Sir, have you obſerved, in the laſt *Gentleman's Magazine*, an account of a *degree of cold*, Decem. 30, near

near Bath, which to me exceeds all reaſonable belief, *viz.* 30 degrees below 0 in Farenheit's ſcale, and this at 10 o'clock *p. m.*?

The morning of December 30. here (at Tooting), a little before day, my thermometer was at 18, *i. e.* 14 degrees below 0; but at night, about 8 o'clock, no more than 8 degrees: But Feb. 7. at near 7 o'clock *a. m.* it ſtood at 14, *i. e.* 18 degrees below 0; which was the greateſt degree of cold I have obſerved in this or any other year.—I ſhould not have mentioned this account from Bath, but that the gentleman, who gives it, ſeems to have a good inſtrument, and mentions his having been converſant with ſuch matters many years. If there be no miſtake, it is an unaccountable phænomenon to me. In all my obſervations, I have found the greateſt degree of cold to be about an hour before ſun-riſing, in *time of froſt*; which is what one would reaſonably expect, *if the froſt holds all night*. I can gueſs at the cauſe of an uncommon *local warmth*, but not ſo of an unuſual degree of cold.

I have had ſome thoughts of laying a few thermometrical obſervations, I have made this ſeaſon, before the *Royal Society*, but know not whether they will be acceptable, from,

SIR,

Your very affectionate humble ſervant,

Tooting, Feb. 13, 1754.

Henry Miles.

LXVI. *A Letter from Monſieur* de L'Iſle, *of the* Royal Academy of Sciences *at* Paris, *to the Reverend* James Bradley, *D. D. Aſtronomer Royal, and* Savilian *Profeſſor of Aſtronomy at* Oxford.

Tranſlated from the French.

SIR, Paris, Nov. 30, 1752.

Read Feb. 21, 1754. I Have received the letter, which you did me the honour of writing to me on the 22d of Auguſt this year, O. S. together with the obſervations made by you at Greenwich, which correſpond with thoſe of Monſieur de la Caille made at the cape of Good Hope, with relation to the parallax of the Moon, Venus, and Mars. I now ſend you, according to your deſire, the compariſon, which I have made between your obſervations and thoſe of Monſieur de la Caille. I have, at preſent, only compared thoſe of Mars; and tho' a conſiderable number of them have been made by you both, yet I find but ſix, which really correſpond, that is, which have been made on the ſame ſtar, and in the ſame night; there being between them only the interval of time neceſſary for paſſing from the meridian of the cape to that of Greenwich. From many obſervations of Monſieur de la Caille, compared with thoſe made in Europe, we are aſſured, that the cape of Good Hope is eaſt of Greenwich $1^h\ 14'$; and this is the difference of longitude, which I have ſuppoſed, in order to reduce your obſervations to thoſe of the cape.

As

As you have not ſet down your firſt obſervations on the ſuperior or northern limb of Mars, as Monſieur De la Caille mention'd, that he would do on his part; I have been obliged to ſuppoſe the apparent diameter of Mars known; and, in the uſe of your firſt obſervations, I have conſidered it, as, or a little leſs than, you found it, when you began to obſerve.

The firſt obſervation, which you made correſponding with that of Monſieur De la Caille, is of Auguſt 31. in the morning, on which day you found, that the center of Mars in the meridian was 11′ 21″ ſouth of ſtar 33 of Piſces. If we deduct 13″ for the apparent ſemidiameter of Mars at that time, 11′ 8″ will be the difference of the declination of the northern limb of Mars and the ſtar in the meridian of Greenwich. The diurnal variation of Mars in the declination was, at that time, 4′ 47″; whence we may conclude it to be 14″ 48‴ for 1ʰ 14′, to be ſubtracted from the diſtance obſerved at Greenwich, in order to reduce it to what it would have been in the meridian of the Cape: So that, by your obſervation, the northern limb of Mars would have been ſouth of the ſtar 10′ 53″ 12‴. Monſieur De la Caille found, that ſtar north of the northern limb of Mars 10′ 18″ 24‴. The difference therefore is 34″ 48‴, for the ſum of the parallaxes of the height of Mars, on that day, in the meridian of Greenwich and the Cape.

Here follows what I have found, with reſpect to the other days.

	′	″	‴
Sept. 4. in the morning, at Greenwich, Rigel ſouth of the centre of Mars . .	7	50	30
Semidiameter of Mars	0	13	00

	′	″	‴
Rigel ſouth of the northern limb of Mars at Greenwich	8	3	30
Diurnal variation in the declination 4′ 8″, anſwering to 1h 14′	0	12	47
Rigel ſouth of the northern limb of Mars by the obſervations of Greenwich reduced to that of the Cape . .	8	16	17
The ſame diſtance obſerved at the Cape	8	51	30
Difference or ſum of the parallaxes of the height of Mars . . .	0	35	13
Oct. 3. in the evening, at Greenwich, the northern limb of Mars was ſouth of the ſtar λ of Aquarius	4	42	30
The diurnal variation of Mars in declination 0′ 59″, anſwering to 1h 14′	0	3	2
On the 6th of September Mars ſouth of the ſtar, reduced to the meridian of the Cape	4	45	32
The diſtance obſerved at the Cape . .	4	9	54
The ſum of the parallaxes of the height of Mars	0	35	38
Oct. 4. in the evening, at Greenwich, the ſtar λ was north of the northern limb of Mars	3	26	0
The diurnal variation in declination 1′ 18′, anſwering to 1h 14′ . .	0	4	1

The ſtar northern Sept. 6. of Mars, reduced to the meridian of the cape	3	30	1
Obſerved at the Cape	2	58	12
The ſum of the parallaxes of the height of Mars	0	31	49

Oct. 7. in the evening, at Greenwich, the ſtar λ ſouth of the northern limb of Mars 2 17 0

The diurnal variation of the declination 2′ 10″, anſwering to 1h 14′ . .	0	6	42
The ſtar ſouth on the 6th of Sept. of Mars, reduced to the Cape . .	2	10	18
Obſerved at the Cape	2	36	36
Sum of the parallaxes of the height of Mars	0	26	18

Oct. 9. in the evening, at Greenwich, the ſtar λ ſouth of the northern limb of Mars 7 35 0

Diurnal variation of the declination 2′ 49″, anſwering to 1h 14′ . .	0	8	42
The ſtar λ ſouth of the northern limb of Mars, reduced to the Cape . .	7	26	18
Obſerved at the Cape	7	57	24
The ſum of the parallaxes of the height of Mars	0	31	6

This ſum of the parallaxes, which I have found from each obſervation, is the angle at Mars formed by the two viſual rays taken by two obſervers of the ſame point of Mars. I afterwards deduced, from every one of theſe ſix obſervations, the horizontal parallax of Mars, by taking it in the ſame ratio to the total ſine, which that angle at Mars is to the ſum of the ſines of the apparent diſtance of Mars in the zenith of each obſerver; and I found the horizontal parallax of Mars as you ſee hereunder.

		"	‴		"	‴	
1751.	Aug. 31.	26	42	the hori-	0	27	additional correc-
	Sept. 14.	27	10	zontal pa-	0	1	tions for reducing
	Oct. 3.	27	35	rallaxes	2	34	the parallaxes to
	4.	24	34	of ♂ for	2	46	what they ought to
	7.	20	20	each ob-	3	20	be in the oppoſition
	9.	27	35	ſervation.	3	46	of Mars to the Sun.

As the horizontal parallax of Mars varies according to the diſtance of Mars from the Earth, I have calculated, by Dr. Halley's tables, the real diſtances of Mars from the Earth at the times of your ſix obſervations above-mentioned, and at the time of the oppoſition of Mars to the Sun; after which, making uſe of the ratio between theſe diſtances, I examined how much the horizontal parallax of Mars muſt be leſs, in each of your obſervations, than at the time of that oppoſition. Theſe are the quantities, which compriſe the ſmall table given above, under the title of corrections. Theſe quantities, being added to the parallax deduced ſimply from each obſervation, have given the horizontal parallax, ſuch as it ought to be in each obſervation reduced to the time of the oppoſition.

The

The calculations being made, here follows what I have found with reſpect to the horizontal parallax of Mars at the time of the oppoſition.

			″	‴	
By the obſervation of	31	Aug.	27	9	
	14	Sept.	27	11	
	3	Oct.	30	9	*
	4		27	20	
	7		23	40	*
	9		27	39	

By taking a mean or arithmetical medium between theſe 6 determinations, we may conclude the horizontal parallax of Mars, at the time of the oppoſition, to be 27″ 11‴: but as there are two determinations, which differ from each other about 3″, and which are thoſe of the 3d and 7th of October; we may reject them, and then we ſhall find the mean between the other four to be 27″ 20‴. Hence you ſee, Sir, that whether we reject thoſe two determinations, or employ them, the horizontal parallax of Mars, at the time of the oppoſition, comes out near 27″ $\frac{1}{4}$; and according to the ratio of the diſtance of the Sun and Mars from the Earth at that time, we conclude the horizontal parallax of the Sun to be about 10″ $\frac{1}{3}$.

This is what I have been able hitherto to conclude from your obſervations of Mars, with reſpect to the parallax of the Sun. Having made the ſame calculations from my own obſervations, and thoſe of other aſtronomers, which I could hitherto collect; I have found very near the ſame parallax of the Sun, by taking

taking a medium between all the obſervations of each aſtronomer. But I have not always found, that the different obſervations of other aſtronomers agree ſo well with each other, as yours do: For which reaſon, I have been a little more doubtful in concluding the true parallax of the Sun; and I have no hopes of being able to determine it more preciſely, than I have done from your obſervations, till I have verified the obſervations of all the other aſtronomers by each other, and rejected thoſe, which ſhall be found evidently faulty, after a rigorous examination of them, which I intend to make.

I have not yet compared your other obſervations with thoſe of Monſieur de la Caille: I propoſe to do it, as ſoon as I ſhall have leiſure. I ſend you, in the mean time, his obſervations, which our Academy has publiſhed, in order to give other aſtronomers the ſatisfaction of comparing them with their own.

I do not doubt, that you, Sir, have thought of the tranſit of Mercury over the Sun, expected on the 6th of May next year. I have made a calculation of it from Dr. Halley's tables, rectified, not only by corrections given by himſelf after the tranſit in 1723, but likewiſe from the obſervations made upon the tranſit in 1740, at Cambridge in New England; ſo that I hope to be exact within a few minutes, with reſpect to the ingreſs and emerſion of Mercury into and from the diſk of the Sun.

I have calculated, that the ingreſs will be at $2^h\ 44'$ in the morning at Paris, and the emerſion at $10^h\ 37'$, and, conſequently, the duration of the paſſage $7^h\ 53'$; and, likewiſe, that Mercury will paſs near the centre of the Sun; which is ſufficient to inform the aſtronomers,

nomers, of the preſent age, of the time, when they are to expect the tranſit: Whereas thoſe, who ſhall make uſe of Caſſini's tables, from which our ephemerides, and thoſe of Italy, are calculated, will be miſtaken no leſs than 4 hours; the tranſit being to happen ſo long ſooner by thoſe tables, than by Dr. Halley's. This difference appeared to me conſiderable enough to deſerve, that aſtronomers ſhould be acquainted with it; and this I am preparing to do by an advertiſement, which will be immediately publiſhed.

In the mean time, I thought it proper to ſend to the Eaſt Indies, to endeavour to procure from thence exact obſervations of the tranſit in thoſe places, where the whole duration may be obſerved. The emerſion may be ſeen, in part of North America, a little after ſun-riſe; and ſince an accurate obſervation, which may be made there, will ſerve to determine the parallax of the Sun, if thoſe obſervations are compared with the others to be made in the Eaſt Indies, I am obliged to write to you, to requeſt you to recommend to all thoſe, who ſhall be able, and have an opportunity to make this obſervation, in your Engliſh colonies, to attend to it; and that the Royal Society will likewiſe make uſe of their intereſt for that purpoſe.

I have found, that at Cambridge near Boſton, where Mr. Winthrop obſerved the tranſit in 1740, Mercury's emerſion from the Sun will be at $5^h\ 45'$ in the morning; which is 51 minutes after ſun-riſe: At New York it will be $5^h\ 32'$, which is 34 minutes after ſun-riſe; in other places, in the neighbourhood, it will be, in proportion, nearer or farther from ſun-riſe; as will be eaſily calculated. But it were to be

wiſh'd,

wiſh'd, that the obſervation ſhould be made principally in thoſe places, the longitude of which is known, with the utmoſt exactneſs poſſible, from other obſervations; which would ſerve the better to determine the parallax of the Sun by the compariſon of thoſe obſervations with ſuch, as ſhall be made in the Eaſt Indies. We already have a conſiderable number of obſervations relating to the longitude of Cambridge and New York, beſides the new ones, which may be ſtill made there; ſo that thoſe two places would be the moſt proper to obſerve the approaching tranſit of Mercury over the Sun; which I deſire you to recommend to the Royal Society, in order that they may exert all their intereſt to procure ſuch obſervations. I am, with perfect eſteem,

SIR,

Your moſt humble, and

moſt obedient ſervant,

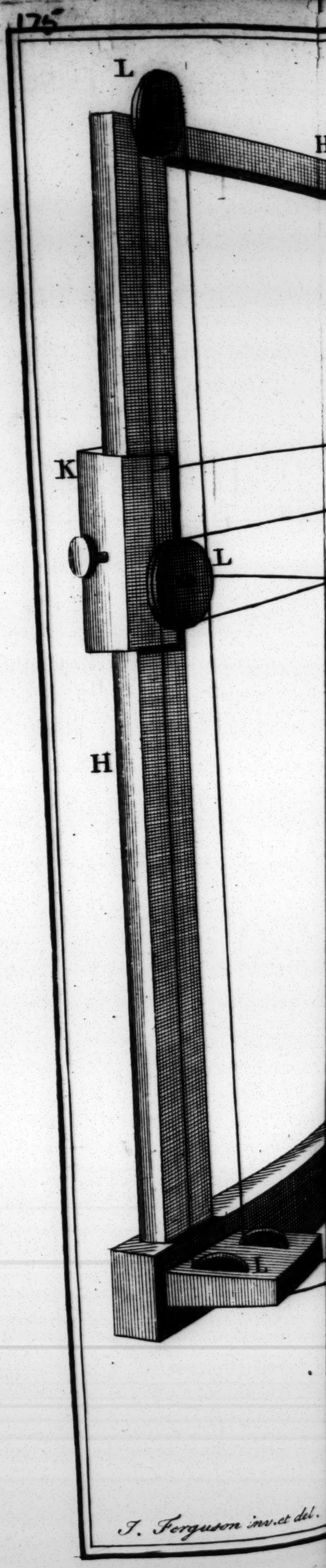
De L'Iſle.

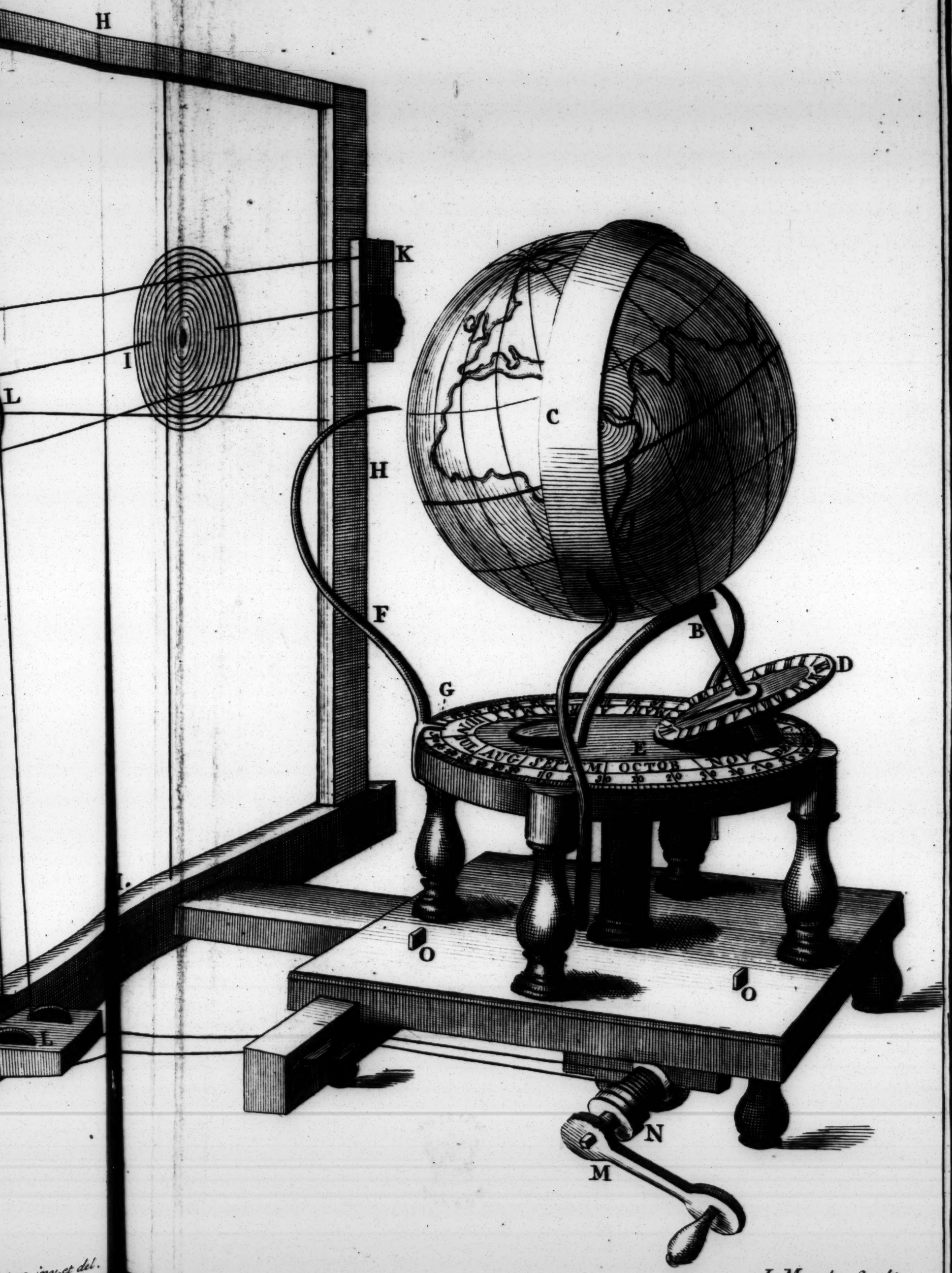
Deſcription of a Piece of Mechaniſm contrived by James Ferguſon, *for exhibiting the Time, Duration, and Quantity, of Solar Eclipſes, in all Places of the Earth.*

Read Feb. 21, 1754.

THE principal Parts of this machine (See *Plate* XIX.) are,

1. A terreſtrial globe turned round, by a handle, on


L
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J. Ferguson inv.et del.



J. Mynde sculp.

on an axis inclined 23½ degrees. On this axis is an index, which goes round a dial-plate of 24 hours in each rotation of the globe.

2. A circular plate, on whoſe limb are inſerted the months and days of the year. This plate ſupports the globe, and, by turning it till any given day comes to a fixed pointer called the annual index, gives the axis of the globe the ſame poſition to the ſun, or to a candle properly placed, that the earth's axis has to the ſun on that day of the year.

3. A penumbra, or thin circular piece of braſs, divided into twelve digits by concentric circles, which repreſent a ſection of the moon's penumbra, and is proportioned to the ſize of the globe; ſo that the ſhadow of this plate, formed by the ſun, or a candle placed at a convenient diſtance, with its rays tranſmitted thro' a convex lens, to make them fall parallel on the plate, covers exactly all thoſe places on the globe, which the moon's ſhadow and penumbra do on the earth. So that any ſolar eclipſe may be ſhewn with this machine, by candle-light, almoſt as well as by the light of the ſun.

4. A frame, on the two upright ſides of which are ſcales of the moon's latitude, or declination from the ecliptic. To theſe ſcales are fitted two ſliders, with indices for adjuſting the penumbra's centre to the moon's latitude, as it is north or ſouth, aſcending or deſcending.

5. A crooked wire, which always points to the middle of the earth's enlightened diſk.

6. A ſolar horizon, dividing the enlightened from the darkened hemiſphere of the globe, and ſhewing at what places the general eclipſe begins, is at the

greateſt obſcuration, or ends, with the riſing or ſetting ſun.

7. A handle, which, by wheel-work, turns the globe round its axis, and at the ſame time, by a thread and pullies, carries the penumbra over its ſurface with a velocity proportional to that of the moon's ſhadow over the earth; which being quicker or ſlower, according to the different diſtances of the moon from the earth, is eaſily regulated, in the machine, by changing one of the pullies.

To rectify the machine for uſe.

The true time of new moon, and her latitude at that time, muſt be firſt known. If her latitude exceeds the number of minutes on the ſcales, there can be no eclipſe of the ſun at that conjunction; but if it does not, the ſun will be eclipſed to ſome places of the earth: And to ſhew the times and various appearances of the eclipſe, at thoſe places, proceed in order, as follows:

If you rectify for performing by the light of the ſun; 1. Move the ſliders till their indices point to the moon's latitude on the ſcales, as it is at that time north or ſouth, aſcending or deſcending. 2. Turn the month-plate till the day of new moon comes to the annual index. 3. Unſcrew the collar on the axis of the handle, and ſet the penumbra, by hand, till its centre comes to the perpendicular thread in the middle of the frame. 4. Turn the handle till the meridian of London, on the globe, comes juſt under the point of the crooked wire; ſtop there, and turn the hour-circle, by hand, till XII. at noon comes to

its index. 5. Turn the handle till the hour-index on its circle points to the time of new moon, and, holding it there, ſcrew faſt the collar on its axis. Laſtly, elevate the machine till the ſun ſhines thro' the two ſight-holes on its pedeſtal; and the whole will be rectified.

To rectify for ſhewing by candle-light, proceed, in every reſpect, as above, ſave in the laſt article; inſtead of which, place a candle before the machine, about four yards from it, ſo as the ſhadow of the interſection of the threads, in the middle of the frame, may fall preciſely on that part of the globe, to which the crooked wire points: Then, with a pair of compaſſes, take the diſtance betwixt the penumbra's centre and interſection of the threads, and equal to that diſtance ſet the candle higher or lower, as the penumbra's centre is above or below the ſaid interſection. Laſtly, Hold a convex lens betwixt the penumbra and candle, ſo as the candle may be in its focus.

Theſe things done, which may be as ſoon as expreſſed, turn the handle backward until the penumbra almoſt touches the right ſide of the frame; then turn it gradually forward, and obſerve the following phænomena:

1. Where the eaſtern edge of the ſhadow of the braſs penumbra firſt touches the globe at the ſolar horizon, thoſe, who inhabit the correſponding part of the earth, ſee the eclipſe begin on the uppermoſt edge of the ſun juſt at the time of its riſing. 2. In that place where the centre of the penumbra firſt touches the globe, the inhabitants have the ſun riſing upon them centrally eclipſed. 3. When the whole pen-

umbra juſt falls upon the globe, its weſtern edge at the ſolar horizon touches and leaves the place, where the eclipſe is ending on the lowermoſt edge of the ſun at its riſing. Continue turning, and, 4. The croſs lines in the centre of the penumbra will go over all thoſe places of the globe, where the ſun is centrally eclipſed. 5. When the eaſtern edge of the ſhadow touches any place of the globe, the eclipſe begins there: When the vertical line in the penumbra comes to any place, then is the greateſt obſcuration at that place; and when the weſtern edge of the penumbra leaves the place, the eclipſe there ends; the times of all which are ſhewn by the index on the hour-circle: And from the beginning to the end the ſhadows of the concentric circles ſhew the number of digits eclipſed at all the intermediate times. 6. When the eaſtern edge of the penumbra leaves the globe under the ſolar horizon, the inhabitants ſee the ſun beginning to be eclipſed on his lowermoſt edge at its ſetting. 7. Where the penumbra's centre leaves the globe, the inhabitants ſee the ſun ſet centrally eclipſed. And, laſtly, where the penumbra is wholly departing from the globe, the inhabitants ſee the eclipſe ending on the uppermoſt part of the ſun's edge at the time of its diſappearing in the horizon.

References to the figure of the machine.

A, the terreſtrial globe. *B*, its axis. *C*, the ſolar horizon. *D*, the hour-circle, with its index. *E*, the month-plate *F*, the crooked wire. *G*, the annual index. *H*, *H*, *H*, *H*, the ſquare frame, the ſcales being on the ſides hid from the view

taken

taken in the drawing. *I*, the penumbra. *K*, *K*, the ſliders. *L*, *L*, *L*, *L*, pullies, over which the line runs, that moves the penumbra. *M*, the handle, to which both ends of the line are fixed. *N*, a collar on the axis of the handle. *O*, *O*, the ſights on the pedeſtal.

LXVIII. *A Letter from the Reverend* Henry Miles, *D. D. F. R. S. to Mr.* John Canton, *F. R. S. concerning the late hard Weather.*

Dear Sir,

Read Feb. 28, 1754. I Thank you for your account of the ſtate of your thermometer, on the days moſt remarkable for cold this ſeaſon; and, in a paper incloſed, have ſent you the regiſter I made of my thermometer (ſee p. 527.), on the ſame days, according to your deſire; which, if you pleaſe, you may lay before the Royal Society, with the few following remarks:

You tell me, you have never before obſerved ſo great a degree of cold; nor have I, for ſeveral years before you began to make your obſervations, or ſince you did. It was in the year 1740. when I, occaſionally, hung a mercurial thermometer abroad; and in February, the following year, conſtantly. Not having heard, that any one had uſed to do ſo, I was led to it, by a very ſenſible warmth in the air, which I perceived upon accidentally looking out at my window, a good while before day: So that I cannot undertake

to

to aſcertain the degree of cold, which we may have had, higher than that time.

The near agreement between your regiſter and mine is remarkable, at noon and night, when the obſervations were made at the ſame hour. The conſiderable difference, on the 7th inſtant, *a. m.* when your thermometer was 6 deg. higher, I attribute, partly, to your obſervation being made an hour later than mine, and after the ſun had been above the horizon three-quarters of an hour: For I have found, by long obſervation, that the coldeſt time of the day, in general, is between an hour, and half an hour, before the ſun riſes. But, perhaps, this difference may likewiſe have been occaſioned, in part, by warm ſteams tranſpiring the earth, at that time; for I have ſeveral times obſerved our ſudden thaws, this winter, to have been attended with circumſtances, which led me to think they had their riſe from ſuch a cauſe. Some mornings, in a great froſt, have been univerſally clear before day; when, having regiſtered the ſtation of the mercury, I afterwards frequently viewed the thermometer, and found it to riſe 3, 4, or more, deg. when another, which I always regiſter with it, and is on the ſame ſide of the houſe, but at a window of a room one ſtory higher, has not riſen at all. From whence I expected a thaw, and never have been deceived. It has, in an hour's time, grown cloudy, and the thaw viſible, and this before ſun-riſing, and without any change in the wind; which maketh it probable, it muſt have been cauſed by a ſubterranean heat, paſſing thro' the ſurface of the ground.

I beg leave to make one obſervation more, which is, that the air in my chamber, which is moſtly ſhut

up,

up, hath not, upon a continued thaw, acquired the ſame degree of warmth with that abroad, ſometimes, under three whole days. I am, dear Sir,

Your, and the Royal Society's

moſt obedient humble ſervant,

Tooting, Feb. 28, 1754.

Henry Miles.

P. S. On Dec. 31. laſt, my thermometer ſtood at the ſame degree, as it did on the 7th inſtant, in the morning; which times were the coldeſt, that I have obſerved this ſeaſon.

Regiſter of the thermometer at Spital Square, in London.		Regiſter of the thermometer at Tooting, in Surrey.	
Feb. 1754.		Feb. 1754.	
Day 6.	At $7^h\frac{1}{2}$ a. m. againſt the houſe 17 and in the garden 15	Day 6.	At $4^h\frac{3}{4}$ a. m. againſt the houſe, clear 15 and never lower after.
	At 2 p. m. againſt the houſe $27\frac{1}{2}$		At 2 p. m. clear - - $28\frac{1}{2}$
	At 8 p. m. - - 21		At 8 p. m. moſtly clear - 20
Day 7.	At 8 a. m. - - 20	Day 7.	At near 7 a. m. clear - almoſt 14
	At 2 p. m. - - 30		At 2 p. m. partly cloudy - 31
	At 8 p. m. - - 26		At 8 p. m. clear - - 26
Day 8.	At 8 a. m. - - 26	Day 8.	At 6 a. m. cloudy - - 23
	At 2 p. m. - - 32		At 2 p. m. cloudy - - 33
	At 8 p. m. - - 28		At 8 p. m. cloudy - - 28
Day 9.	At 8 a. m. - - 26	Day 9.	At 6 a. m. cloudy - - 23
	At 2 p. m. - - $35\frac{1}{2}$		At 2 p. m. cloudy - - $36\frac{1}{2}$

The Reverend Dr. Bradley having obſerved his thermometer, at the Royal Obſervatory at Greenwich, to ſtand at $14\frac{1}{2}$, on the 6th inſt. about $7^h\frac{1}{2}$ a. m. it may be preſumed to have ſtood ſome deg. lower before that time of the morning.

LXIX.

LXIX. *A* Catalogue *of the* Fifty Plants *from* Chelsea-Garden, *presented to the* Royal Society *by the worshipful Company of* Apothecaries, *for the Year* 1753. *pursuant to the Direction of Sir* Hans Sloane, *Bart. Med. Reg. & Soc. Reg. nuper Præs.*; *by* John Wilmer, *M. D. Clariss. Societatis Pharmaceut. Lond. Soc. Hort.* Chelf. *Præf. & Prælect. Botan.*

Read March 7, 1754.

1551. ANagallis cærulea foliis binis terniſve ex adverſo naſcentibus *C. B.*

1552. Arctotis foliis ovatis dentatis petiolis longiſſimis ſuperne dentatis caule ramoſo *Flor. Leyd. Prod.* Anemonoſpermos Africana foliis Plantaginis flore ſulphureo *Com. rar. Pl. fig.* 35.

1553. Aſtragalus cauleſcens procumbens leguminibus ſcrotiformibus inflatis mucronatis piloſis *Hort. Upſal.*

1554. Atriplex ſylveſtris fructu roſeo compreſſo vel ſtellato *C. B. P.*

1555. Baccharis foliis lanceolatis longitudinaliter dentato ſerratis *Hort. Cliff.* Senecio Afric. arboreſcens folio ſerrato *Boerh.* 117.

1556. Calendula foliis linearibus denticulatis & integerrimis *Flor. Leyd.* Dimorphotheca ſtatices folio *Vaillant.*

1557. Carduus Luſitanicus luteus foliis coronopi glabris & rigidis *Tourn. Inſt. R. H.*

1558. Celtis fructu obſcure purpuraſcente *Inſt. R. H.* 612.

1559. Centaurea calycibus duplicato-ſpinoſis ſubrotundis, foliis decurrentibus, lanceolatis ſinuoſis inermibus floribus lateralibus *Hort. Cliff.*

1560. Centaurea calycibus ſubulato-ſpinoſis foliis decurrentibus, inermibus lanceolatis inferioribus dentatis *H. Cliff.*

1561. Centaurea calycibus ciliatis, foliis tomentoſis pinnatifidis, foliolis ovatis integerrimis exterioribus majoribus *Hort. Cliff.* Jacea Epidaurica candidiſſima & tomentoſa *Tourn.* 445.

1562. Cerinthe Alpina perennis flore ſtriato *Tourn. Inſt. R. H.* Glaſtum montanum *Dalechampii Lugd.* 1204.

1563. Clinopodium montanum *C. B. P.* Acinos Auſtriaca *Cluſii.*

1564. Cynanchum caule volubili, fol. reniformibus cordatis acutis *Hort. Cliff.* Periploca Monſpeliaca folio rotundiore *Tourn.* 93.

1565. Delphinium nectariis diphyllis labellis bifidis apice barbatis, foliis inciſis, caule erecto *H. Upſal.* Delphinium Orientale, Aconiti fol. fl. cærul *T. Cor.* 30.

1566. Dioſma foliis ſubulatis acutis *Hort. Cliff.* Spiræa Africana foliis cruciatim poſitis *Com. rar. pl.*

1567. Echinopus minor *J. B.* 3. 72.

1568. Elichryſum ſylveſtre latifolium, fl. parvo ſingulari ſtœchadi citrini alteri inodore Lobelii *Tourn. Inſt. R. H.* Affinis capitulis brevioribus *J. B.* 3. 157.

1569. Epilobium foliis lineari-lanceolatis integerrimis, floribus corymboſis terminatricibus.

1570. Eryngium Americanum fœtidum Ituba Braſil. *Hort. Leyd.* 237.

1571. Galenia *Hort. Cliff.* Sherardia *Pontider.*

1572. Geniſta tinctoriâ latifolia Lucenſis *J. B.*

1573. Heſperis ſicula fruteſcens ſiliqua tricuſpidi *Tourn.* Leucoium minus fruticans purpureum, montanum crucigerum *Boc.* p. 2. 148. *T.* 3.

1574. Hieracium incanum lanuginoſum Raguſinum, piloſellæ flore *Hort. Lugd. App.*

1575. Hippocrepis leguminibus ſolitariis ſubſeſſilibus *Hort. Cliff.* Ferrum equinum ſiliqua ſingulari *Tourn.* 400.

1576. Hyoſeris foliis glabris haſtato-pinnatis *Flor. Leyd. Prod.* Dens leonis minor foliis radiatis *C. B. P.*

1577. Kempferia *Hort. Cliff.* Aroorchis tuberoſo platyphyllos *Breyn.*

1578. Leonuros Canadenſis origani folio *Tourn. Inſt. R. H.* 187. Origanum fiſtuloſum Canadenſe *Cornuti.* Monarda *Hort. Cliff.*

1579. Malope foliis ovatis crenatis glabris *Hort. Cliff.* Malacoides betonicæ folio *Tourn. Inſt.* 98.

1580. Marrubiaſtrum vulgare *Tourn.* Sideritis alſines triſſaginis fol *C. B. P.* 233.

1581.

1581. Mercurialis caule fruticoſo, foliis tomentoſis *Hort. Cliff.*

1582. Nerium Indicum anguſtifolium, floribus odoratis ſimplicibus *Hort. Leyd.*

1583. Othonna foliis infimis lanceolatis integerrimis, ſuperioribus ſinuato-dentatis *Hort. Cliff.* Jacobæa Africana fruteſcens coronopi folio *Com. Hort. Amſt.* p. 2. 139.

1584. Papaver caule unifloro nudo hiſpido, foliis pinnato-ſinuatis *Hort. Upſ.*

1585. Perſicaria fruteſcens maculoſa Virginiana flore albo *Park. Theat.* 857.

1586. Phyſalis radice perenne foliis cordatis obtuſis *H. Cliff.* Alkekengi Bonarienſe repens bacca turbinata viſcoſa *Hort. Eltham.* p. 11.

1587. Plumbago foliis petiolatis *Hort. Cliff.* Ceylanenſis fol. ſplendente Ocymaſtri fl. lacteo Burman *Zeyl.* 195.

1588. Salvia foliis ovatis utrimque acuminatis ſerratis *H. Cliff.* Sclarea Mexicana altiſſima facie Heliotropii *Hort. Elt.* 339.

1589. Sanicula Offic *C. B. P.* 319. Mas Fuchſii ſive Diapenſia *J. B.* 3. 639.

1590. Saxifraga Alpina Ericoides flore cæruleo *Tourn. Inſt. R. H.* Sedum Alpinum Ericoides cæruleum *C. B. Prod.* 132.

1591. Scabioſa corollulis quinquefidis foliis inciſis, caule fruteſcente *Hort. Cliff.* Scabioſa Africana fruteſcens *Herman. Par.* 219.

1592. Seſeli petiolis ramiferis membranaceis oblongis integris *Hort. Cliff.* Fæniculum ſylveſtre elatius Ferulæ, fol. longiori *Tourn.* 311.

1593. Solanum Campechianum calycibus echinatis *H. Elt.* 360.

1594. Soldanella *Hort. Cliff.* Alpina rotundifolia *C. B. P.* 295.

1595. Sonchus floribus ſolitariis, foliorum laciniis extrorſum flexis *Hort. Cliff.* Sonchus Tingitanus papaveris folio *T.* 475.

1596. Teucrium foliis lanceolatis integerrimis petiolatis, ſpicis laxis ſubrotundis *Hort. Cliff.* Polium lavendulæ folio *C. B. P.* 220.

1597. Thevetia *Hort. Cliff.* Ahovas major *Piſ.*

1598. Tordylium umbellulis remotis, foliis pinnatis pinnis ſubrotundis laciniatis *Hort. Cliff.*

1599. Veronica ſpicata lanuginoſa & incana floribus cæruleis *Amman. Ruth.*

1600. Zigophyllum foliis ſubſeſſilibus *Hort. Cliff.* Fabago Africana arboreſcens flore ſulphureo, fructu rotundo *Com. Rar.* 10.

LXX. *An Account of ſome Experiments upon a Machine for meaſuring the Way of a Ship at Sea.* By Mr. J. Smeaton, *F. R. S.*

Read April 4, 1754.

IN the *Philoſophical Tranſactions*, N° 391. for November 1725. Mr. Henry de Saumarez gives an account of a machine for meaſuring a ſhip's way more exactly than by the log. This machine conſiſts of a firſt mover, in the form of the letter Y. Upon the two arms of the Y are faſtened two vanes, inclined in ſuch a manner, that when the

the Y is hauled through the water by a rope, faſtened to the ſtem or tail thereof, it may turn round, and, of conſequence, endeavour to turn the rope round. The other end of the rope, being faſtened to the end of a ſpindle capable of moving freely round, will be made to do ſo by the rotations of the Y, communicated to the rope. A motion being thus communicated to a ſpindle within the ſhip, this ſpindle may be made to drive a ſett of wheel-work, which will regiſter the turns of the Y; and the value of a certain number of theſe turns being once found, by proper experiments, they are eaſily reducible into leagues and degrees, &c. The only difficulty then is, whether this Y will make the ſame number of rotations in going the ſame ſpace, when it is carried through the water faſt, as when it is carried ſlow. Upon this head Mr. de Saumarez, as well in the paper abovecited, as in a ſubſequent one publiſhed in *Philoſ. Tranſ.* N° 408. for March 1729. has given an account of ſeveral trials, which he has made of it, from which it appears, that this machine, in part, anſwers the end propoſed; and is, in part, defective: The errors of which he ſuppoſes to proceed from the ſinking down of the Y into the water, upon a ſlow motion; the axis of its rotation being then more oblique to the horizon than in a quick one.

In a machine, conſtructed like this, it is evident, that the end of the ſpindle, to which the rope is faſtened, muſt be of ſufficient ſtrength and thickneſs, not only to bear the force or ſtreſs, that the hauling of the Y through the water will lay upon it, in the greateſt motion of a ſhip; but alſo to bear the accidental jerks, that the waves will ſuperadd thereto.

The

The thickneſs of the ſpindle then being determined by theſe conditions; it is alſo manifeſt, that, to prevent the ſpindle from being pulled out of its place by the draft of the rope, there muſt be a ſhoulder formed upon it, which muſt be greater than the part of the ſpindle before deſcribed, for the ſpindle to bear againſt. The ſize, that Mr. Saumerez propoſes to give to his Y, is 27 inches the whole length; 15 inches for the length of the arms (which are to be opened to a right angle); 8 inches for the length of each vane; 4 inches and an half broad, and the ſtems and ſhank to be two-thirds of an inch thick. According to theſe dimenſions, the reſiſtance, that this part of the machine will meet with, in paſſing thro' the water, will, in the ſwift motions of the ſhip, be very conſiderable: conſequently, the neceſſary bulk of the pivot-end of the ſpindle, and its ſhoulder, will occaſion a conſiderable friction in the turning thereof, and retardation to the rotation of the machine.

To cure theſe defects, as much as poſſible, inſtead of the Y before deſcribed, I made trial of a ſingle plate of braſs, of about 10 inches long, 2 and an half broad, one-thirtieth of an inch thick, and cut into an oval ſhape. This plate being ſet a little atwiſt, and faſtened by one end to a ſmall cord, in the manner of the Y, is likewiſe capable of making a rotation, in being drawn through the water; but with this difference, that as this is but a ſmall thin plate drawn edgeways through the water, its reſiſtance, in paſſing through it, is much leſs; of conſequence, a much ſmaller line is ſufficient to hold it, which again conſiderably diminiſhes the reſiſtance; and this, of courſe, proves a double diminution of friction in the

ſpindle:

ſpindle: Firſt, as the preſſure upon it is leſs; and, ſecondly, as it allows the ſpindle and ſhoulder to be of a leſs diameter. To break the jerks of the waves; next to the end of the ſpindle I fixed a ſpiral ſpring of wire, to which the cord was faſtened; which, by this means, was capable of playing backwards and forwards, and giving way to the irregularities of the ſea: and, leſt the plate ſhould lay faſt hold of any thing, or any extraordinary jerk ſhould damage the ſpindle or ſpring, a knob, or button, was faſtened upon the cord, at a ſmall diſtance from the ſpring, which ſtopped upon a hole in a piece of wood, and prevented the ſpring from being pulled out to above a certain length; ſo that all addition of force, beyond this, could only tend to break the cord, and carry away the plate. The ſpindle, being thus guarded from accidents, will allow of a ſtill further diminution of its ſize; ſo that, at laſt, I ventured to make the ſpindle-pivot no more than one-twentieth of an inch diameter, and that of the ſhoulder one-eighth; being of tempered ſteel, and ſufficiently ſmooth. The hole in which the pivot, and againſt which the ſhoulder worked, was of agate likewiſe, well poliſhed.

Being thus provided, in May 1751. I procured a boat, upon the ſerpentine river in Hyde-park, to try how far the turns of the machine would be conſiſtent with themſelves, when the ſame ſpace was meaſured over with the ſame, and with different velocities. The courſe was determined at each end, by obſerving the coincidence of two trees, in a line nearly at right angles to the river. We, however, rowed beyond the

mark, that the machine might be in full play when the courſe was begun: The ſpindle was ſtopped at the beginning and end; the numbers read off, and were as follows:

The ſpace between the marks was, by eſtimation, about half a mile.

			Revol.
1ſt	rowing up the river, in	11 min. the plate made	615
2d	down	14 . . .	645
3d	up	18 and an half .	612
4th	down	9 and an half .	603
5th	up	18 . . .	620
6th	down	10 . . .	600

It is obſervable, that the greateſt difference, among the above obſervations, is between the 2d and 6th, being 645 and 600; the difference being about one fourth part of the whole; the times being 14 minutes and 10, both in going down the river: Whereas thoſe obſervations, which differ moſt in point of time *viz.* the 3d and 4th, being performed in 18 minutes and an half, and 9 minutes and an half, reſpectively; have their revolutions more nearly alike, being 612 and 603; which differ only by one ſixty-eighth of the whole. From theſe obſervations I was led to think, that the different velocities, wherewith a veſſel moves forwards, would make no material difference in the number of rotations of the plate; or, at leaſt, that thoſe differences would be leſs than the irregularities ariſing from other cauſes, even in trials nearly ſimilar.

The

The next trial of this machine was on board a ſmall ſailing veſſel, in company with Dr. Knight, and Mr. William Hutchinſon, an experienced ſeaman, and maſter of a conſiderable merchant-ſhip. Our expedition was upon the river Thames, and ſome leagues below the Nore. The intention of the trial here was, to find, in general, how far it agreed with the log, and how it would behave in the ſwell of the ſea; a compariſon with the meaſure of a real diſtance being here impracticable, on account of the tides and currents.

The method of trial was this: We ſuffered the whole log-line to run out, being 357 feet between the firſt knot and the end. The perſon, who hove the log, gave notice, at the extremes of this meaſure, that the perſon, who attended the dial of the machine, might ſtop the ſpindle at the beginning and end; while a third obſerved, by a ſeconds-watch, the time taken up in running theſe 357 feet. By theſe means, we were enabled to aſcertain the comparative velocity, wherewith we moved, and the number of turns of the plate at each trial, correſponding to 357 feet by the log; which, if the machine and log were both accurate, ought to have been always the ſame. The particulars of theſe experiments are contained in the following table.

Turns of the Plate.		Seconds of time during the running out of 357 feet of log-line
83	In the river at anchor by the tide 124	
82	The ſame repeated . . . 134	
81	Sailing in the river . . . 98	
79	In the river at anchor by the tide 135	
76	Sailing in the river . . . 115	
74	At ſea upon a wind . . . 64	
74	The ſame repeated . . . 69	
71	Sailing in the river . . . 71	
70	The ſame 66	
70	Before the wind at ſea . . 77	
70	The ſame 56	
70	The ſame 52	
66	Before the wind in the river . 55	
64	The ſame 53	
64	The ſame 60	
64	The ſame 43	
63	At ſea upon a wind . . 53	
62	The ſame 52	
62	Sailing in the river . . 45	

It appears from theſe trials, made in different poſitions of the veſſel with regard to the wind, both in the river and at ſea, as well by the tides at anchor, as in ſailing, that the turns of the plate, correſponding with the ſpace of 357 feet by the log, were from 62 to 83; and the times, in which this ſpace was run, were from 45 to 135 ſeconds; the greater number of revolutions anſwering to the greater number of ſeconds or ſlower movement of the veſſel. Upon finding this conſiderable diſagreement between the log and plate, when ſwift and ſlow motions are compared, I did not ſuppoſe, that they proceeded from a retard-

retardation of the plate in ſwift motions, but from the hauling home of the log in ſlow ones. As for inſtance; the log, to do its office accurately, ought to remain at reſt in the water, whatever be the motion of the veſſel. But even the keeping the line ſtrait, and much more the ſuffering the log to haul the line off the reel (as practiſed by many), will make the log, in ſome meaſure, follow the veſſel, and will be greater, in proportion as the time of continuance of this action is greater; and therefore the log will follow the ſhip twice as far in going one knot, when the ſhip is twice as long in running it. The conſequence of this is, that a veſſel always runs over a greater ſpace than is ſhewn by the log-line; but that this error is greater, in proportion as the veſſel moves ſlower. It is this reaſon, I ſuppoſe, that has induced the practical ſeamen to continue the diſtance between their knots ſhorter than they are directed by the theory.

Afterwards, in the ſame ſummer, I made ſuch another expedition, in a ſailing veſſel, along with captain Campbell of the Mary yacht, and Dr. Knight. Having prepared two of theſe machines as near alike as poſſible, I determined to try, how far they were capable of agreement, when expoſed to the ſame inconveniencies, and uſed together. During the trial of theſe machines, one made 86,716 revolutions, and the other made 88,184. During this ſpace, they were compared at ten ſeveral intervals. The revolutions between each interval differed from the proportion of theſe numbers, in the firſt compariſon, one-nineteenth of the whole interval. The errors of each interval, in the other compariſons, were, in order, two-ſeventeenths, one-nineteenth, one-twentieth, one-

fifty-fourth, one-fourteenth, one-eightieth, one-ſixty-ſeventh, one-fourteenth, one-ſixteenth; the greateſt errors being where the ſpaces were the ſhorteſt. In other reſpects, the plates ſeemed to perform their duty, in the water, well enough, tho' the ſea was as rough, in this voyage, as our ſmall veſſel would well bear.

Laſtly, Being, for ſome time, on board the Fortune ſloop of war, commanded by Alexander Campbell, Eſq; in company with Dr. Knight, for the purpoſe of making trial of his new-invented ſea-compaſſes, I had frequent opportunities of making uſe of theſe machines, by comparing them with one another, with the log, and with real diſtances; and having, by repeated trials, pretty well aſcertained the numb r of turns of the plate, that was equal to a given ſpace, by the help of the log, in the manner before deſcribed, when the ſhip was upon a middle velocity; I found the ſpaces, ſo meaſured, nearly conſiſtent with themſelves, and with the truth: But all this while the winds and weather were very moderate. It afterwards happened, that we run 18 leagues in a briſk gale of wind, which, tho' not fair for us (being before the beam), yet drove us ſometimes at the rate of 8 knots an hour, as appeared by heaving the log. During this run I obſerved, that the reſiſtance of the water, to the line and plate, was very conſiderable, and increaſed the friction of the ſpindle ſo much, as to prevent it from beginning to turn, till the plate had twiſted the line to ſuch a degree, that when it did ſet a going, it would frequently run 150 or 200 turns at once. I alſo obſerved, that the wind coming acroſs the courſe of the ſhip, blew the cord a good deal out of the direction of the ſpindle, and

cauſed

cauſed the line to rub againſt the ſafeguard-hole, for the button to ſtop againſt, as above deſcribed; which undoubtedly occaſioned conſiderable friction in that place. But the moſt untoward circumſtance, that I obſerved, was, that being in a rough, but ſhort chopping ſea, and ſailing obliquely acroſs the waves, the plate would frequently be drawn from one wave to another through the air, without touching the water; and, as it appeared, would jump from one wave to another, the unevenneſs of the ſurface, joined to the quickneſs of the motion, not permitting the plate to follow the depreſſion of the water. This evil I endeavoured to remedy, by placing upon the line, at a ſmall diſtance before the plate, ſome hollow bullets, ſuch as are made for nets, in order to keep the plate ſo low down in the water, as to be below the bottom of the waves. This, in part, I found they did; but they, at the ſame time, added ſo much reſiſtance, in their paſſing through the water, that the inconvenience was as great one way, as the other.

Upon making up the account of this run, I found the number of rotations were leſs, by one full third, than they ought to have been, compared with former obſervations; which afforded me a convincing proof, that this inſtrument was conſiderably retarded in quick motions.

The length of the line made uſe of was about 20 fathoms, which I found neceſſary, that the water, diſturbed by the body of the ſhip, might be tolerably ſettled before the plate was drawn through it; but this length of line was alſo an inconvenience, as it met with greater reſiſtance in the water.

Upon

Upon the whole, it ſeems to me, that an inſtrument, made as above deſcribed, is capable of meaſuring the way of a ſhip at ſea, when its velocity does not exceed 5 ſea miles an hour, to a degree of exactneſs exceeding the log. It therefore may be uſeful in the menſuration of the velocities of tides, currents, &c. and alſo in meaſuring diſtances at ſea in taking ſurveys of coaſts, harbours, &c. Thus far it ſeems capable of performing, upon the ſuppoſition, that it cannot be brought to a greater degree of perfection. But this I am very far from ſuppoſing: On the contrary, I do not deſpair, that it may be brought to anſwer the end of meaſuring the way of a ſhip at ſea univerſally; and, for that reaſon, it may not be amiſs to put down a few hints, concerning the cauſe and cure of the errors above-mentioned, for the ſake of thoſe, who may hereafter be inclined to proſecute theſe enquiries*.

It appears then from the preceding obſervations, that the rotation of the plate is conſiderably retarded in the quickeſt motions of the ſhip; and ſenſibly ſo, in all velocities exceeding 5 miles an hour. This may proceed, firſt, from the friction of the machine increaſing in a greater proportion than the power to turn it round. Secondly, From the water's being put in motion by the ſhip, ſo as to follow it in the ſame

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* Upon communicating theſe experiments and obſervations to my ingenious friend Mr. William Ruſſel, he gave me an account of a machine, that he had made trial of in a voyage, ſome years ſince, from the Levant, ſo nearly agreeing with the above-deſcribed, that one would have imagined we had been of each other's council in deſigning them.

di-

direction, and that to a conſiderable diſtance aſtern. And, Thirdly, from the plate's jumping from wave to wave, when their concavity is great, and diſtance little.

The firſt may, in ſome meaſure, be helped, by applying a loaded fly, of a proper ſize, to the ſpindle of the machine, which will prevent its ſticking faſt for a time, and then whirling round with great rapidity, as it is apt to do when the reſiſtance is great; by which means, the motion will be rendered more equal and uniform, as was juſtly obſerved to me by my friend Mr. Ellicott of this Society.

Alſo, if the body of the machine were hung, equally poized, upon croſs-centres, like thoſe uſed for ſea-compaſſes, or in the manner of a ſwivel-gun, as captain Alexander Campbell well propoſed; the ſpindle of the machine would readily place itſelf in the ſame direction with the line that draws it, and thereby avoid unneceſſary frictions from the oblique direction of the cord.

The ſecond may be helped by placing the machine upon the end of a pole, faſtened near the forecaſtle, over the ſide of the ſhip. By this means, a ſhorter line will be neceſſary, and the plate prevented from working in the more diſturbed water at the ſtern.

Laſtly, Its quitting the water, perhaps, might be helped by joining a ſhank of braſs, of ſix inches long, and three-quarters of an inch diameter, to the fore-part of the plate, to which the cord muſt be faſtened, the ends of the ſhank being formed into a figure moſt convenient for paſſing thro' the water with eaſe. The weight of this will cauſe the fore-part of the plate to

ſink faſter than the other, and endeavour to give it a direction down into the water *.

I had intended to have made trial of the effect of theſe alterations, but have been prevented, partly by want of opportunity, and partly from the indifference, with which I found ſuch a contrivance as this, even if brought to perfection, was likely to be received by ſeamen; who, in general, do not ſeem to be over fond of making trial of new inſtruments, eſpecially if propoſed by landmen, as, in deriſion, they are pleaſed to call us.

Indeed it may be objected, that, could we meaſure the way of a ſhip thro' the water ever ſo exactly, unleſs ſome method were found out, of aſcertaining the currents, *&c.*; a ſhip's courſe, with reſpect to the globe, could not hereby be determined. But then it may be replied, with equal juſtice, that it is for want of a means of meaſuring the way of a ſhip thro' the water (and this compared with other check obſervations), that the drift and velocities of the principal currents have not already been determined.

Mr. de Saumarez, in his ſecond paper, of March 1729. makes mention of another machine for this purpoſe, which he himſelf acknowleges to be inferior to his former, eſpecially in rough weather at ſea. But as ſeveral others have fallen upon, and propoſed, a machine ſimilar to this; it may not be amiſs to add the following remarks upon it. The firſt mover, in this, is compoſed of four arms, fixed to the bottom of a perpen-

* Mr. Ruſſel's plate was joined to a ſhank, who never found it to jump out of the water, at any time, when he made uſe of it.

perpendicular ſpindle; each arm is furniſhed with a vane, which opens one way, and ſhuts the other, as ſome have attempted the making of horizontal windmills. This, by being carried thro' the water progreſſively, will turn round, and the faſter, as the ſhip moves faſter: But to judge, whether it will do it proportionably in all velocities of the ſhip, let us conſider,

1. That a good ſailing ſhip will frequently ſail at the rate of 10 ſea miles (60 to a degree) an hour, which is at the rate of 17 feet *per* ſecond.

2. Suppoſing the ſide of the fly, where the vanes are cloſed, to be retained by the water at reſt; the oppoſite ſide of the fly, where the vane is open, muſt meet the water with a velocity double to that of the ſhip, or at the rate of 34 feet in a ſecond; as would be the caſe with the upper part of a coach-wheel, whoſe velocity thro' the air is double to that, wherewith the coach moves forward.

3. That a plane ſurface of 3 inches ſquare, moving thro' the water with a velocity of 34 feet *per* ſecond, will meet with a reſiſtance, at leaſt, equal to 70 pounds avoirdupoize.

4. That the reſiſtance, which the open vanes will meet with in the water, will, in ſwift motions, be very conſiderable, and, of conſequence, the fly will move much ſlower than it ought to do, if theſe reſiſtances were leſs.

5. That from hence there is much reaſon to doubt, whether the reſiſtance of the medium, and friction of the machine, taken together, will always produce ſuch diminution, in the number of turns, as that the number of revolutions, actually ſhewn by the indexes,

may be the ſame, when the ſame ſpace is gone over with a great velocity, as with a ſmall one.

LXXI. *Obſervationes Eclipſium Satellitum Jovis habitæ Ulißipone in Regali Collegio Beatiſſimæ Virginis à neceſſitatibus, dictæ à Joanne Chevalier, Præſbytero Seculari Congregationis Oratorii, Anno* 1753.

Read Feb. 14, 1754.

DIE 30 Aprilis, cœlo clariſſimo, obſervavi, teleſcopio Gregoriano 6 pedum longitudinis, emerſionem primi ſatellitis è umbra Jovis, hora poſtmeridiana temporis veri 9^h $57'$ $48''$.

Die 24 Maii, iterum cœlo puriſſimo, obſervavi, eodem teleſcopio Gregoriano, emerſionem tertii ſatellitis ab umbra Jovis, hora poſtmeridiana temporis veri 8^h $19'$ $6''$.

LXXII. *Obſervatio Solis defectus Ulißipone habita, in Æde Beatiſſimæ Virginis à neceſſitatibus, nuncupatæ à Joanne Chevalier, Præſbytero Congregationis Oratorii, die 26ª Octobris* 1753.

Read Feb. 14, 1754.

HUNC ſolis defectum commodè, ac exactè obſervare potui, cœlo clariſſimo ac puriſſimo. Initium ac finem eclipſis teleſcopio 15 palmorum examinavi, digitos vero obſcurationis

rationis micrometro menſus ſum. Eodem tempore ſodalis meus phyſicæ profeſſor, eaſdem eclipſis phaſes obſervabat in ſpecie ſolis, in charta alba depicta, per alium tubum duabus lentibus inſtructum.

	Temp. ver. ant. merid. h	′	″
Initium eclipſis . . .	7	31	4
2 digiti obſcurationis . .	7	43	54
4 digiti	7	56	40
6 digiti	8	9	30
8 digiti	8	22	10
10 digiti	8	34	40
Major obſcuratio undecim digitorum	h	′	″
ac 5 min.	8	41	46
8 digiti	9	2	0
2 digiti	9	44	20
Finis	9	58	18

Prope tempus maximæ obſcurationis, luce ſolis notabiliter imminutâ, Jovem, Venerem, aliquaſque ſtellas, primæ ac ſecundæ magnitudinis, videre licuit. Mercurium tamen, propter ejus ad ſolem appropinquationem, aſpicere non potui.

Speculum catoptricum diametri trium palmorum, in cujus foco plumbum liqueſcit, & ex ligno flamma ſtatim excitatur, eadem phænomena efficiebat, cum ſolis digiti ſeptem jam obſcurati eſſent: poſt tempus tamen majoris phaſis, lignum in foco ſuſpenſum per aliquod temporis intervallum minimè combuſſit.

Tempore majoris obſcurationis aer frigeſcere cæpit,

valido flante vento à ſetemptrione; craſſique vapores aſcendere viſi ſunt è fluvio portuque vicino.

In thermometro domini de Reaumur, ſequentes mutationes obſervatæ fuere.

7^h 20′ Spiritus vini 12° & dimidium altitudinis ſupra terminum congelationis aquæ obtinebat.

h	′	°
8	12	13
8	30	$12\frac{1}{2}$
8	43	$12\frac{1}{4}$
8	55	$11\frac{3}{4}$
9	30	12
10	0	$12\frac{1}{2}$
10	40	$13\frac{1}{4}$

LXXIII. *An Account of ſome aſtronomical Obſervations taken at* Liſbon *by M.* John Chevalier *in the Year* 1753. *By* James Short, *M. A. and F. R. S.*

Read March 14, 1754.

THIS gentleman mentions two emerſions of the ſatellites of Jupiter, *viz.* one of the firſt, and another of the third, both obſerved, in a very clear air, with a Gregorian teleſcope ſix feet long. Dr. Bevis, from a great number of obſervations, has computed *formulæ* of tables for the times of the immerſions and emerſions of the firſt ſatellite of Jupiter, and which times we have ſeldom found to differ from the obſervations above 10″: By com-

comparing, therefore, the time of the emerſion of the firſt ſatellite obſerved by this gentleman, with the time computed from theſe *formulæ*, the difference of longitude between London, at St. Paul's, and the place of obſervation at Liſbon, comes out to be 36′ 6″; and by ſeveral former correſponding obſervations the difference had been found to be 36′ 10″. By ſome obſervations of the ſame ſatellite, ſent me lately by the reverend P. Pezenas at Marſeilles, and which he had received from ſome aſtronomers at Liſbon, the difference of longitude between London and Liſbon is ſometimes 34′, and ſometimes 35′: But it is to be remarked of theſe gentlemen, that tho' they both obſerved at the ſame place, and thro' refracting teleſcopes of the ſame length; yet they ſometimes differ from one another a whole minute, in the time of emerſion.

M. Chevalier further mentions the obſervation of the eclipſe of the ſun laſt October, thro' a teleſcope of 15 palms. He ſaw both the beginning and end, in a very clear air; and ſays, that the greateſt quantity of the eclipſe was 11 digits and 5 minutes, which he meaſured with a micrometer; but, unluckily, has not given us either the diameter of the ſun, or that of the moon, which he might have meaſured (for the eclipſe was annular), tho' he was at the pains of meaſuring all the digits, both in the increaſe and decreaſe of the eclipſe. He further takes notice, that, at the time of the greateſt obſcuration, the light of the ſun was remarkably diminiſhed; and that they were able to ſee Jupiter, Venus, and ſome ſtars of the firſt and ſecond magnitude; but he could not ſee Mercury, on account of

of his proximity to the ſun: And that a reflecting ſpeculum, of three palms in diameter, which could melt lead, when placed in its focus, and inſtantly ſet wood in a flame; did produce the ſame effects, even when the ſun was ſeven digits eclipſed; but that, about the time of the greateſt obſcuration, it was not able to burn wood, tho' held in its focus for ſome time: And that, at the ſame time, the air became very cold, the wind blowing hard from the north; and that ſome vapours, or fog, were ſeen to riſe out of the river and adjacent harbour.

He likewiſe mentions ſome alterations in the ſpirit of wine thermometer of M. de Reaumur, during the eclipſe.

The ſame eclipſe of the ſun was obſerved alſo at Liſbon by A. P. Euſebius da Veiga, profeſſor of mathematics. His times of the beginning and end are ſomewhat earlier than thoſe of M. Chevalier; and he alſo makes the greateſt obſcuration larger, by 3 minutes of a digit.

To his account of this eclipſe he ſubjoins ſome occultations of ſtars by the moon, obſerved by him at Liſbon laſt year; *viz.* of Venus, on the 27th of July; of *α Libræ*, on the 5th of Auguſt; and of *β Capricorni*, on the 5th of October.

LXXIV. *An*

LXXIV. *An Explanation of an Inſtrument for meaſuring ſmall Angles, the firſt Account of which was read before the* Royal Society May 10, 1753. *By Mr.* John Dollond. *In a Letter to* James Short, *M. A. and F. R. S.*

SIR,

Read Apr. 25, 1754. THE account which I gave you, ſome time ago, of a new micrometer, was contained in as few words as poſſible; being rather deſirous, that experiments might be made, before I ſaid much concerning it: But ſince your many repeated experiments have confirmed what was expected from it, I have endeavoured to draw up a more full account of this inſtrument, with demonſtrations of the principles which it is founded upon, which I here ſend you inclos'd, and which you may lay before the Royal Society, if you think proper. I am, Sir,

Your moſt obedient humble ſervant,

Denmark-Court, April 4. 1754. John Dollond.

BEfore I enter upon particulars relating to this micrometer, it will be proper to make a few preparatory obſervations on the nature of ſpherical glaſſes, ſo far as may be neceſſary to render the following explanation more eaſily underſtood.

Obſervation 1. It is a property of all convex ſpherical glaſſes to refract the rays of light, which are tranſmitted through them, in ſuch a manner, as to collect all thoſe that proceed diverging from any one point

point of a luminous object, to ſome other point; whoſe diſtance from the glaſs depends chiefly on its convexity, and the diſtance of the object from it.

Obſ. 2. The point, where the rays are thus collected, may be conſidered as the image of that point, from which they diverge. For if we conceive ſeveral radiant points thus emitting rays, which, by the refractive quality of the glaſs, are made to converge to as many other points; it will be an eaſy matter to underſtand, how every part of the object will be truly repreſented. As this property of ſpherical glaſſes is explained and demonſtrated by all the writers on optics; it being the very foundation of the ſcience, the bare mention of it is ſufficient for the preſent purpoſe.

Obſ. 3. It will be neceſſary, however, to obſerve farther, that the lines connecting every point in the object with its correſponding ones in the image, do all interſect in a certain point of the axis or line paſſing thro' the poles of the glaſs, where its two ſurfaces are parallel, and may be properly called its centre: Whence it appears, that the angles ſubtended by the object and its image from that point, muſt be equal: And therefore their diameters will be in the ſame ratio, as their diſtances from that point.

Obſ. 4. As the formation of the image by the glaſs depends entirely on the property above-mentioned, that is to ſay, its collecting all the light, that is incident on it, from the ſeveral points of the object into as many other points at its focus; it follows, that any ſegment of ſuch a glaſs will alſo form an image equal, and every way ſimilar, to that exhibited by the whole glaſs; with this difference only, that it will be ſo much darker, as the area of the ſegment is leſs than that of the whole glaſs.

Obſ.

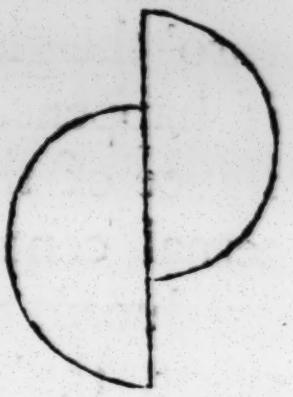
Obſ. 5. The axis of a ſpherical glaſs is a line connecting the centres of the ſpheres, to which the two ſurfaces are ground; and where-ever this line paſſes thro' the glaſs, there the ſurfaces are parallel. But if it happens, that this line does not go thro' the ſubſtance of the glaſs, ſuch a glaſs is ſaid to have no internal centre; but it is conceived to be in its plane produced, till it meets the axis: And this imaginary point, tho' external to the glaſs, is as truly its centre, and is as fixed in its poſition to it, as if it were actually within its ſubſtance.

Obſ. 6. If a ſpherical glaſs, having its center or pole near its middle or centre of its circumference, ſhould be divided by a ſtrait line through the middle; the centre will be in one of the ſegments only. For how exact ſoever a perſon may be ſuppoſed to be in cutting it thro' the centre; yet 'tis hard to conceive, how a mathematical point ſhould be divided in two: Therefore the centre will be internal to one of the ſegments, and external to the other. But if a ſmall matter be ground away from the ſtrait edge of each ſegment, both their centres will become external; and ſo they will more eaſily be brought to a coincidence.

Obſ. 7. If theſe two ſegments ſhould be held together, ſo as to make their centres coincide; the images, which they give of any object, will likewiſe coincide, and become a ſingle one. This will be the caſe, when their ſtrait edges are joined to make the glaſs, as it were, whole again: But let the centres be any-how ſeparated, their images will alſo ſeparate, and each ſegment give a ſeparate and diſtinct image of any object, to which they may be expos'd.

Obſ. 8. Tho' the centres of the ſegments may be drawn from their coincidence, by removing the ſeg-

ments in any direction whatever; yet the moſt convenient way for this purpoſe is, to ſlide their ſtrait edges one along the other, till they are removed, as the figure in the margin repreſents them: For thus they may be moved without ſuffering any falſe light to come in between them. And by this way of removing them, the diſtance between their centres may be very conveniently meaſured; *viz.* by having a Vernier's diviſion, commonly, tho' falſely, call'd a Nonnius's, fixed to the braſs-work, that holds one ſegment, ſo as to ſlide along a ſcale on the plate, to which the other part of the glaſs is fitted.



Obſ. 9. As the images of the ſame object are ſeparated, by the motion of the ſegments; ſo thoſe of different objects, or different parts of the ſame object, may be made to coincide. Suppoſe the ſun, moon, or any planet, to be the object; the two images thereof may, by this contrivance, be removed, till their oppoſite edges are in contact: In which caſe, the diſtance between the centres of the two images will be equal to the diameter of either; and ſo of any other object whatever.

Obſ. 10. This divided glaſs may be uſed, as a micrometer, three different ways. In the firſt place, it may be fixed at the end of a tube, of a ſuitable length to its focal diſtance, as an object-glaſs; the other end of the tube having an eye-glaſs fitted as uſual in aſtronomical teleſcopes. Secondly, It may be applied to the end of a tube much ſhorter than its focal diſtance, by having another convex glaſs within the tube, to ſhorten the focal diſtance of that, which is cut in two. Laſtly, It may be applied to the open end of a reflecting

flecting teleſcope; either of the Newtonian, Gregorian, or Caſſegrain conſtruction. And tho' this laſt method is much the beſt, and moſt convenient, of the three; yet, as the firſt is the moſt natural, as well as the eaſieſt to be underſtood; it will be proper to explain it fully, and to demonſtrate the principles, on which this micrometer is conſtructed, by ſuppoſing it made uſe of in the firſt way: Which being done, the application of it to other methods will be readily underſtood.

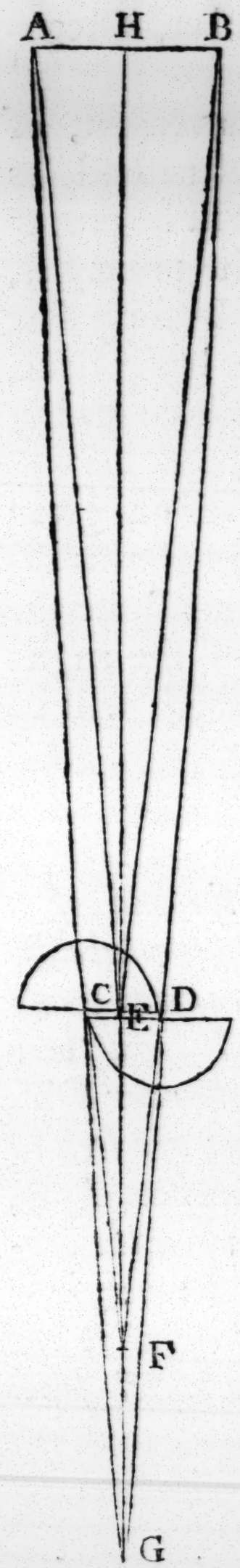
Having thus, by the foregoing obſervations, given a general idea of the nature and effects of this divided object glaſs, I ſhall proceed to demonſtrate the principles, from whence the meaſures of the angles are to be obtained by this inſtrument; which will be done by the following propoſitions.

Proposition I.

Suppoſe a divided object-glaſs fixed at the end of a tube, according to the firſt method, and the tube directed to the object intended to be meaſured; and ſuppoſe, likewiſe, the ſegments removed from their original poſition, in the manner directed under Obſ. 8. *till the oppoſite edges of the two images are ſeen in contact at the focus of the eye-glaſs: Then, I ſay, the angle ſubtended, by the diſtance between the centres of the ſegments, from the focus of the eye-glaſs, where the edges are ſeen in contact, is equal to the angle ſubtended by the diameter of the object from that ſame point.*

Demonſtration.

Let the line *A B* (ſee the next page) repreſent the diameter of the object to be meaſured; and the



points *C D* the centres of the two glaſs ſegments: Alſo *G* the focus where the images of the extremities of the object are coincident. It is evident, from *Obſ.* 3. that *A G* and *B G* are ſtrait lines, that paſs thro' the centres of the ſegments, and connect the extreme points of the object with their correſponding points in the images; and therefore, as the diameter of the object and the diſtance between the centres of the ſegments are both inſcribed between theſe two lines, they muſt needs ſubtend the ſame angle from the point where thoſe lines meet; which is at *G*. *Q. E. D.*

The focal diſtance *C G*, or *D G*, is variable, according to the diſtance of the object from the glaſs: So that it decreaſes as the diſtance of the object from the glaſs increaſes; and when the object is ſo far off, that the focal length of the glaſs bears no proportion to its diſtance; then will it be leaſt of all, as *C F* or *D F*; and the point *F* is call'd the focus of parallel rays. Any other focus, as *G*, being the focus of a near object, is call'd a reſpective focus; as it reſpects a particular diſtance: But the focus of parallel rays reſpects all objects that are at a very great diſtance; ſuch as is that of all the heavenly bodies.

PRO-

Proposition II.

The diſtance HE *of the object from the glaſs, is to* EF, *the focal diſtance of parallel rays, as the diſtance* HG *of the object from its image is to* EG, *the diſtance of the image from the glaſs: That is,* HE : EF :: HG : EG.

The demonſtration of this propoſition may be gathered from any treatiſe of dioptrics; it being a general rule for finding the reſpective focus to any given diſtance, when the focus of parallel rays is known.

Proposition III.

The angle ſubtended by the diameter of the object, from the glaſs, is equal to that ſubtended, by the opening of the centres of the ſegments, from the focus of parallel rays. That is, the angle AEB *equal to the angle* CFD.

Demonſtration.

It appears, by inſpection of the figure, that $AB : CD :: HG : EG$.

And by the laſt propoſition $HE : EF :: HG : EG$.

Then, as the two laſt terms of theſe two analogies are alike; the two firſt terms of one will be in the ſame proportion as the two firſt terms of the other; which gives the following proportion: $AB : CD :: HE : EF$. Whence the truth of the propoſition is evident.

From

From this propoſition it appears, that the angle ſubtended by the diameter of the object from the glaſs, is found without any regard to the diſtance of the object, or to the diſtance of the reſpective focus, where the image is ſeen; as the meaſure depends entirely upon the focus of parallel rays and the opening of the ſegments. We may likewiſe, from hence, derive a rule for the quantity of the angle, without conſidering the length of the glaſs. Let an object, whoſe diameter is known, be ſet up at ſome known diſtance; the angle it will ſubtend from the glaſs may then be found by trigonometry: Then let it be meaſured by this micrometer, and the diſtance, between the centres of the ſegments, found on the ſcale already mentioned, will be the conſtant meaſure of the ſame angle, in all other caſes: Becauſe the diſtance of the object makes no alteration in the meaſure of the angle, as has been demonſtrated: And thus having obtain'd the diſtance between the centres of the ſegments, which anſwers to any one angle, all other diſtances may be computed by the rule of three.

All that has been hitherto ſaid relates to the firſt method of uſing this micrometer; that is, by fitting it to the end of a tube ſuited to its focal length, and by viewing the images with a proper eye-glaſs, in the manner of an aſtronomical teleſcope. But the length of the tube, in this way, would be very troubleſome; and therefore it will be proper to conſider other methods, for an eaſier management. I ſhall, therefore, proceed to the ſecond method, mention'd in *Obſ.* 10. which is, by uſing another object-glaſs to ſhorten the focus of that which ſerves for the micrometer. To

facilitate the underſtanding of this method, it will be neceſſary to premiſe the following obſervation.

Obſ. 11. Rays of light, which are brought to ſuch convergency as to form the image of an object, proceed, after that, diverging, in the manner they did when they iſſued from the object before they were tranſmitted thro' the glaſs; and therefore they may be again collected by another ſpherical glaſs, ſo as to form a ſecond repreſentation of the ſame object; which may again be repeated by a third glaſs, *&c.* So that the firſt image may be conſider'd as an object to the ſecond glaſs, and the ſecond image will be an object to the third, and ſo on. Tho' theſe images may be very different, in reſpect to their magnitudes, yet they will be all ſimilar; being true repreſentations of the ſame object: This will hold good, tho' the ſecond glaſs ſhould be put ſo near the firſt as to receive the rays before the image is form'd: For as the rays are tending to meet at a certain diſtance, the ſecond will receive them in that degree of convergency, and, by an additional refraction, bring them to a nearer focus; but the image will ſtill be ſimilar to that which would have been made by the firſt glaſs, if the ſecond had not been there.

Upon this principle all refracting teleſcopes are made; ſome of which are a combination of four, five, or ſix, glaſſes. The firſt glaſs forms an image of the object; the ſecond repeats the image, which it receives from the firſt; and ſo on, till the laſt glaſs brings a true repreſentation of the object to the eye. The ſame may be ſaid of reflecting teleſcopes: For a ſpherical mirror acts in the ſame manner, in that reſpect, as a ſpherical glaſs.

Now

Now let this be applied to the ſubject in hand. Suppoſe the focal diſtance of the divided object-glaſs to be about forty feet; and ſuppoſe the ſegments to be open'd wide enough to bring the oppoſite edges of an object in contact: Then let another object-glaſs, uncut, be fix'd within the tube, of a proper degree of convexity, to ſhorten the focus of the other as much as may be requir'd; ſuppoſe to twelve feet: By what has been juſt now obſerv'd, this glaſs will repreſent the two images in the ſame form which would have been exhibited by the divided glaſs, if this other glaſs had not been there. For tho' the images are not yet form'd, when the ſecond glaſs receives the rays; yet, as thoſe rays are converging towards it, the ſecond glaſs muſt repreſent thoſe images in the ſame poſition, and form, as the tendency of the rays requires. For while the ſegments are fix'd in their poſition to each other, their images will alſo be fix'd in their poſition; and let them be repeated ever ſo many times, by refraction thro' ſpherical glaſſes, or by reflexion from ſpherical mirrors, they can ſuffer no alteration in their poſition to one another. By this means, the teleſcope may be ſhorten'd, at pleaſure, tho' the ſcale for the meaſure of the angles will remain the ſame. The only inconvenience, which the ſhortneſs of the teleſcope introduces, is a want of ſufficient diſtinctneſs; which will ſo far hinder the exactneſs of the obſervation, as the contact of the edges cannot be ſo accurately determin'd, as they might be with longer teleſcopes.

This difficulty is intirely remov'd by fixing the divided glaſs at the end of a reflecting teleſcope: For the reflexions and refractions, which the rays muſt undergo

undergo in paſſing thro' the teleſcope, will no way alter the poſition of the images which the rays, that have paſſed thro' the ſegments, are tending to: For, as has been already obſerv'd, a number of reflexions and refractions may repeat the images, and alter their magnitudes; but can make no alteration in their proportions.

Therefore this way of fixing the divided glaſs to a reflecting teleſcope, which was the third method propos'd, is, by far, the beſt; as ſuch teleſcopes of moderate and manageable lengths, when well made, are capable of magnifying conſiderably, and ſhewing objects to great advantage. This micrometer's being applicable to the reflecting teleſcope, with ſo much certainty, is no inconſiderable advantage: For any one will eaſily underſtand, that, to meaſure the diameter of a planet exactly, it is neceſſary, that the planet be magnify'd, and ſhew'd diſtinctly, which could not be obtain'd, in the common way, without very great lengths; ſuch as render'd it very difficult, not to ſay impracticable, to take exact meaſures. Beſides, the common micrometer is limited, in this reſpect, upon another account; *viz.* becauſe the diameter of the planet cannot be meaſur'd, without having the whole planet within the field of the teleſcope which confines the magnifying power within very narrow bounds; whereas, by this method, nothing more is requir'd, than to ſee the contact of the edges, which allows the magnifying power to be increas'd at pleaſure.

In the common micrometer, the object is to be taken between two wires, ſo that the contact of its edges with thoſe wires cannot be obſerv'd at one view;

and the leaſt motion of the teleſcope, whilſt the obſerver is turning his eye from one wire to the other, muſt oblige him to repeat the obſervation; whereas, by this method, the contact of the edges of the images is not at all affected by the motion of the teleſcope. Whence the compariſon of this micrometer, with the common ſort, in this reſpect, ſtands thus: The one requires great ſteadineſs in the teleſcope, but yet it is applicable to none, but ſuch as are very difficult to keep ſteady; the other does not require ſuch ſteadineſs, tho' it is applicable to ſhort teleſcopes, which are eaſily managed.

Theſe advantages not only add to the certainty of the obſervation, but aſſiſt vaſtly in the expedition; for an obſerver may make twenty obſervations, in this way, where he could ſcarce, with much fatigue, be ſure of one with the common micrometer. Expedition in making obſervations, muſt be allowed a very great advantage, in this climate, where the uncertainty of the weather renders aſtronomical obſervations ſo precarious, that no opportunities, even the moſt tranſient, ſhould be let ſlip An inſtance of this was given in to the Royal Society, in an account of the eclipſe of the ſun laſt October.

As the motion of the teleſcope gives the obſerver no great inconvenience, in this method; neither does the motion of the object at all diſturb his obſervation (I mean ſuch a motion, as that of the heavens is). This gives him leave to take the diameter of a planet, in any direction; or the diſtance between two ſtars or planets, let their ſituation be how it will; in which reſpect the common micrometer is abſolutely defective; as it can give no angles, but ſuch as are perpendicular

pendicular to the line of their motion; tho' the diameters of the planets, in other directions, is very much wanted; it being highly probable, from the laws of motion, and what we ſee in Jupiter, that ſuch planets, as revolve round their axes, have their polar diameters ſhorter than their equatorial ones.

The diſtances of Jupiter's ſatellites from one another, or from Jupiter's body, cannot be meaſured, with any certainty, in the common way, as their poſition is always very far from being at right angles with the line of their motion: Neither can the moon's diameter, which muſt be taken from horn to horn, ſcarce ever be obtained that way, becauſe it very rarely happens, that the diameter, to be meaſured, lies at right angles to the line of her motion. The ſame may be ſaid of the diſtance between two ſtars. But this micrometer gives angles, in every direction, with equal eaſe and certainty; the obſervation being alſo finiſhed in an inſtant, without any trouble or fatigue to the obſerver. For as there are no wires made uſe of, this way, in the field of the teleſcope; ſo the obſerver has no concern about any illumination. The largeneſs of the ſcale deſerves alſo to be taken notice of, as it may, in this micrometer, be increas'd almoſt at pleaſure, according as the ſmalneſs of the object requires. Another inconvenience attending the common micrometer is, the variation of the ſcale, according to the diſtance of the object. As the teleſcope muſt be lengthen'd, or drawn out farther, for ſhort diſtances; the ſcale, which depends upon that length, is thereby increas'd; which renders the meaſure of the angle very uncertain: Whereas, in this micrometer, the ſcale is the ſame at all diſtances; ſo that the angle may be meaſured with the utmoſt cer-

and the leaſt motion of the teleſcope, whilſt the obſerver is turning his eye from one wire to the other, muſt oblige him to repeat the obſervation; whereas, by this method, the contact of the edges of the images is not at all affected by the motion of the teleſcope. Whence the compariſon of this micrometer, with the common ſort, in this reſpect, ſtands thus: The one requires great ſteadineſs in the teleſcope, but yet it is applicable to none, but ſuch as are very difficult to keep ſteady; the other does not require ſuch ſteadineſs, tho' it is applicable to ſhort teleſcopes, which are eaſily managed.

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tainty, without any regard to the diſtance of the object.

Upon the whole, it may be concluded, that this micrometer is a complete inſtrument in its kind; having many advantages above the common ſort, without any of their diſadvantages: And there is no doubt, but, when brought into practice, it will tend much to the advancement of aſtronomy.

LXXV. *An Account of an Earthquake felt at* York *on the* 19*th of* April 1754. *In a Letter from Mr.* David Erſkine Baker *to* Tho. Birch, *D. D. Secret. R. S.*

Dear Sir,

Read Apr. 25, 1754. ON Friday night laſt, the 19th inſtant, at about eleven o'clock, we were alarm'd, in this city, with the ſhock of an earthquake. As I was myſelf in London, and felt both the ſhocks, which happen'd there in the year 1750, I became immediately ſenſible of what it was. In the room, in which I was ſitting, which was on a firſt floor, the tremulous, or rather undulating, motion of the floor was very plain; and the windows rattled, as if they had been ſhaken by a ſudden ſquall of wind. The ſhock laſted for about three ſeconds, and was attended, or rather preceded, by a rumbling noiſe, not much unlike that made by an empty hearſe driven over a ſtone pavement, and, indeed, exactly the ſame with that, which I remember to have heard, with both the ſhocks, in London, in 1750. The violence of the

ſhock

ſhock ſeem'd to me to be nearly the ſame with that of the 8th of February, but not ſo great as that of the 8th of March, in that year; and the courſe of it, as far as I have been enabled to judge, not only from my own obſervation at the time, but from the accounts of others, ſeem'd to be nearly from ſouth-weſt to north-eaſt, it being felt the ſtrongeſt in the ſtreets which run in that direction; particularly in Stonegate, where I lodge; in Mary-gate, and Lopp-lane; in the latter of which, I was told, that a chamber-bell, in a gentleman's houſe, was heard to ring very diſtinctly; and a gentlewoman of my acquaintance, in a ſtreet, which runs parallel to theſe, who was very weakly, and was ſitting up after a lying-in, was almoſt thrown forwards out of her chair by the ſhock. As I have no great correſpondence in this county, and am, moreover, going to leave it in about a fortnight's time, I fear I ſhall not be enabled to trace out the extent of this ſhock; tho' I am inform'd, it was felt very ſenſibly at Foforth, Biſhopthorpe, Huntington, and Heſslington, ſmall towns at two or three miles diſtance from this city; and alſo at Selby, about ten miles to the ſouthward of York. If you ſhould think this account worth communicating to your illuſtrious Society, you are entirely at liberty ſo to do; and I beg you would believe, that I am, with the utmoſt ſincerity and reſpect, Sir,

Your and their

moſt obedient humble ſervant,

York, April 21, 1754.

David Erſkine Baker.

P. S.

P. S. Since the writing of the above, I have been inform'd, that, in two or three houſes, ſinging birds, which were at the time rooſting in their cages, were thrown off their perches by the ſuddenneſs of the ſhock.

LXXVI. *An Inveſtigation of ſome Theorems which ſuggeſt ſome remarkable Properties of the Circle, and are of Uſe in reſolving Fractions, whoſe Denominators are certain Multinomials, into more ſimple ones.* By *Mr.* John Landen.

Read May 2, 1754. THAT the principal theorems, below inveſtigated, will be of conſiderable uſe in the doctrine of fluxions, by rendering, in many caſes, the buſineſs of computing fluents more eaſy, will, on peruſal, be obvious to every one acquainted with that branch of ſcience. Therefore, as the facilitating computations in that doctrine (which affords us wonderful aſſiſtance in many phyſical enquiries) may be a means of extending our knowlege in philoſophy; it is preſum'd, that this paper will not be thought unworthy the notice of the Royal Society.

I.

Suppoſing $\frac{n\dot{x}}{\sqrt{x^2-1}} = \frac{\dot{y}}{\sqrt{y^2-1}}$, where $\dot{x}$ and $\dot{y}$ denote the fluxions of the variable quantities x and y re-

reſpectively, and n an invariable quantity; it is propos'd to find, in terms of y and z, the equation of which z is a root, and $z^2 - 2xz + 1 = 0$, a diviſor.

Taking the fluents of the given fluxionary equation, we have, ſuppoſing $x = 1$ when y is $= 1$, hyp. log. of $\overline{x + \sqrt{x^2 - 1}}^n$ = hyp. log. of $y + \sqrt{y^2 - 1}$, or $\overline{x + \sqrt{x^2 - 1}}^n = y + \sqrt{y^2 - 1}$: Whence, by ſubſtituting for x its value $\frac{z^2+1}{2z}$ (found by the equation $z^2 - 2xz + 1 = 0$), we have $z^n = y + \sqrt{y^2 - 1}$: Therefore $z^n - y$ is $= \sqrt{y^2 - 1}$; and, ſquaring both ſides, $z^{2n} - 2yz^n + y^2 = y^2 - 1$. Conſequently $z^{2n} - 2yz^n + 1$ is $= 0$; which, ſuppoſing n a poſitive integer, is the equation ſought.

Now it is obvious, n being ſuch an integer, that this equation will have as many trinomial diviſors, of the form $z^2 - 2xz + 1$, as there are values of x correſponding to a given value of y: Which values of x, when y is not greater than 1, nor leſs than -1 (the only caſe I purpoſe to conſider), will not be readily obtain'd from the equation $\overline{x + \sqrt{x^2 - 1}}^n = y + \sqrt{y^2 - 1}$ found above: But, if we multiply the given fluxionary equation by $\frac{1}{\sqrt{-1}}$, we get $\frac{n\dot{x}}{\sqrt{1-x^2}} = \frac{\dot{y}}{\sqrt{1-y^2}}$; of which the equation of the fluents is $n \times$ circ. arc. rad. 1. coſine x = circ. arc rad. 1. coſine y; where x is $= 1$ when y is $= 1$, agreeable to the ſuppoſition we made

made above when we took the fluents of the given fluxionary equation by logarithms. Therefore if A be put for the leaſt arc whoſe coſine is y, and C for the whole circumference, radius being 1; y being the coſine of A, $A+C$, $A+2C$, $A+3C$, &c. x will be the coſine of $\frac{A}{n}$, $\frac{A+C}{n}$, $\frac{A+2C}{n}$, &c. . . . . to $\frac{A+\overline{n-1}\times C}{n}$.

Conſequently, expreſſing the laſt-mention'd coſines, or the ſeveral values of x, by p, q, r, s, &c. $z^{2n} - 2yz^n + 1$ will be $= z^2 - 2pz + 1 \times z^2 - 2qz + 1 \times z^2 - 2rz + 1$, &c. ($n$), when n is a poſitive integer (as we ſhall always ſuppoſe it to be), let z be what it will.

Hence may be eaſily deduc'd a demonſtration of that remarkable property of the circle firſt diſcover'd by Mr. Cotes: But as that property has already been demonſtrated by ſeveral mathematicians, I ſhall omit taking any farther notice of it, and proceed in the inveſtigation of ſome other uſeful theorems which I do not find have ever yet been publiſh'd.

II.

If y be $= 1$; then, A being $= 0$; p, q, r, &c. will be the coſines of $\frac{0}{n}$, $\frac{C}{n}$, $\frac{2C}{n}$, $\frac{3C}{n}$, &c. (n) reſpectively: Therefore p will be $= 1$; and, if n be an even number, one of the coſines q, r, s, &c. will be $= -1$, one of the arcs $\frac{C}{n}$, $\frac{2C}{n}$, $\frac{3C}{n}$, &c. being then $= \frac{C}{2}$.

III.

III.

If y be $= -1$; then, A being $= \frac{C}{2}$; p, q, r, s, &c. will be the cosines of $\frac{C}{2n}$, $\frac{3C}{2n}$, $\frac{5C}{2n}$, &c. (n) respectively: Therefore, if n be an odd number, one of those arcs will be $\frac{C}{2}$, whose cosine is -1.

IV.

If in the equations $z^{2n} - 2yz^n + 1 = 0$, and $z^2 - 2xz + 1 = 0$, we substitute $v-1$ for z, they become $\overline{v-1}^{2n} - 2y \times \overline{v-1}^{n} + 1 = 0$, and $\overline{v-1}^{2} - 2x \times \overline{v-1} + 1 = v^2 - \overline{2+2x} \times v + 2 + 2x = 0$. Consequently

$$\left.\begin{array}{r} v^{2n} - 2nv^{2n-1} + \ldots + 2n \times \frac{2n-1}{2} v^2 - 2nv + 1 \\ \ldots \mp 2yn \times \frac{n-1}{2} v^2 \pm 2ynv \mp 2y \\ + 1 \end{array}\right\} =$$

$$\overline{v^2 - \overline{2+2p} \times v + 2 + 2p} \times \overline{v^2 - \overline{2+2q} \times v + 2 + 2q} \times$$

$\overline{v^2 - \overline{2+2r} \times v + 2 + 2r}$ &c. (n); where, of the two signs prefix'd to the terms where y is a factor, the upper or lower takes place, according as n is an even or an odd number. Whence, by the nature of equations, it follows, that $\overline{2+2p} \times \overline{2+2q} \times \overline{2+2r}$, &c. is $= 2 \mp 2y$. But this equation vanishing when y is $= 1$ and n an even number, or when y is $= -1$

and n an odd number, it will be proper to conſider thoſe two caſes more particularly.

V.

Firſt, Let us ſuppoſe $y = 1$, and n an even number: Then p being $= 1$, and one of the other coſines q, r, s, &c. $= -1$ (*Art.* II.); we ſhall have $v^{2n} - 2nv^{2n-1} + \ldots + n^2 v^2 = \overline{v^2 + 0} \times \overline{v^2 - 4v + 4} \times \overline{v^2 - 2 + 2q} \times v + 2 + 2q \times \overline{v^2 - 2 + 2r} \times v + 2 + 2r$, &c. Therefore dividing by v^2, $v^{2n-2} - 2nv^{2n-3} + \ldots + n^2 = \overline{v^2 - 4v + 4} \times \overline{v^2 - 2 + 2q} \times v + 2 + 2q \times \overline{v^2 - 2 + 2r} \times v + 2 + 2r$, &c. that factor in which the value of the coſine q, or r, &c. is -1, being expung'd.

Conſequently n^2 is $= 4 \times 2 + 2q \times 2 + 2r \times 2 + 2s$, &c. when the factor, whoſe value is nothing, is expung'd.

VI.

Let us now ſuppoſe $y = -1$, and n an odd number: Then one of the coſines p, q, r, &c. being $= -1$ (*Art.* III.), $v^{2n} - 2nv^{2n-1} + \ldots + n^2 v^2$ will be $= \overline{v^2 + 0} \times \overline{v^2 - 2 + 2p} \times v + 2 + 2p \times \overline{v^2 - 2 + 2q} \times v + 2 + 2q$, &c. Therefore, dividing by v^2, $v^{2n-2} - 2nv^{2n-3} + \ldots + n^2$ will be $= \overline{v^2 - 2 + 2p} \times v + 2 + 2p \times \overline{v^2 - 2 + 2q} \times v + 2 + 2q$, &c. and conſequently $n^2 = 2 + 2p \times 2 + 2q \times 2 + 2r$, &c. when the factor, whoſe value is nothing, is expung'd.

VII.

VII.

Subſtituting in the equations $z^{2n} - 2yz^n + 1 = 0$, and $z^2 - 2xz + 1 = 0$, $\frac{a+\omega}{a-\omega}$ inſtead of z, we have

$$\left|\frac{a+\omega}{a-\omega}\right|^{2n} - 2y \times \left|\frac{a+\omega}{a-\omega}\right|^{n} + 1$$

$$= \frac{\overline{a+\omega}^{2n} - 2y \times \overline{a+\omega}^{n} \times \overline{a-\omega}^{n} + \overline{a-\omega}^{2n}}{\overline{a-\omega}^{2n}} = 0, \text{ and}$$

$$\left|\frac{a+\omega}{a-\omega}\right|^{2} - 2x \times \frac{a+\omega}{a-\omega} + 1$$

$$= \frac{\overline{a+\omega}^{2} - 2x \times \overline{a+\omega} \times \overline{a-\omega} + \overline{a-\omega}^{2}}{\overline{a-\omega}^{2}}$$

$$= \frac{\overline{2+2x} \times \omega^2 + \frac{1-x}{1+x} a^2}{\overline{a-\omega}^{2}} = 0. \quad \text{Conſequently}$$

$\overline{a+\omega}^{2n} - 2y \times \overline{a+\omega}^{n} \times \overline{a-\omega}^{n} + \overline{a-\omega}^{2n}$ will be $=$

$\overline{2+2p} \times \overline{2+2q} \times \overline{2+2r}$, &c. $\times \omega^2 + \frac{1-p}{1+p} a^2$

$\times \omega^2 + \frac{1-q}{1+q} a^2 \times \omega^2 + \frac{1-r}{1+r} a^2$, &c.

But, by *Art.* IV. $\overline{2+2p} \times \overline{2+2q} \times \overline{2+2r}$, &c. is $= 2 \mp 2y$, the upper or lower of the two ſigns

 prefix'd

prefix'd to y taking place according as n is an even or an odd number.

Therefore $\overline{a+\omega}^{2n} - 2y \times \overline{a+\omega}^{n} \times \overline{a-\omega}^{n} + \overline{a-\omega}^{n}$ is $=$

$$\overline{2+2y} \times \omega^2 + \frac{1-p}{1+p} a^2 \times \omega^2 + \frac{1-q}{1+q} a^2 \times \omega^2 + \frac{1-r}{1+r} a^2,$$

&c.

Now p being the cosine of any number of degrees, radius being 1, $\frac{1-p}{1+p} a^2$ will be the square of the tangent of half so many degrees, radius being a: Therefore, denoting that tangent by b; and the tangents of half the arcs described with the radius a, whose cosines, when the radius is 1, are q, r, s, &c. being denoted by c, d, e, &c. respectively; we have $\overline{a+\omega}^{2n} - 2y \times \overline{a+\omega}^{n} \times \overline{a-\omega}^{n} + \overline{a-\omega}^{2n} = \overline{2 \mp 2y} \times \overline{\omega^2 + b^2} \times \overline{\omega^2 + c^2} \times \overline{\omega^2 + d^2}$, &c. But when y is $= 1$, and n an even number; or $y = -1$, and n an odd number; $2 \mp 2y$ being $= 0$; nothing can be determin'd from that equation: Wherefore, in those cases, recourse must be had to what is done above.

VIII.

Let us suppose $y = 1$, and n an even number: Then the equation $\overline{a+\omega}^{2n} - 2y \times \overline{a+\omega}^{n} \times \overline{a-\omega}^{n} + \overline{a-\omega}^{2n} = \overline{2+2p} \times \overline{2+2q} \times \overline{2+2r}$, &c. $\times \omega^2 + \frac{1-p}{1+p} a^2 \times \omega^2 + \frac{1-q}{1+q} a^2 \times \omega^2 + \frac{1-r}{1+r} a^2$, &c.

becomes

becomes $\overline{a+\omega}^{2n} - 2 \times \overline{a+\omega}^{n} \times \overline{a-\omega}^{n} + \overline{a-\omega}^{2n} = 4 \times \overline{2+2q} \times \overline{2+2r}$, &c. $\omega^2 \times \omega^2 + \frac{1-q}{1+q} a^2 \times \omega^2 + \frac{1-r}{1+r} a^2$, &c. p being $= 1$ (*Art.* II.) and $\frac{1-p}{1+p} a^2 (= b^2) = 0$.

Moreover, one of the other cosines q, r, s, &c. being $= -1$ (*Art.* II.), some one of the factors $2 + 2q$, $2 + 2r$, $2 + 2s$, &c. will vanish: Which factor being expung'd from the product $4 \times \overline{2+2q} \times \overline{2+2r}$, &c. and restor'd to the divisor $\omega^2 + \frac{1-q}{1+q} a^2$, or $\omega^2 + \frac{1-r}{1+r} a^2$, &c. from which it was taken, that divisor will become $4a^2$; and the product $4 \times \overline{2+2q} \times \overline{2+2r}$, &c. will then (by *Art.* V.) be $= n^2$.

Consequently $\overline{a+\omega}^{2n} - 2 \times \overline{a+\omega}^{n} \times \overline{a-\omega}^{n} + \overline{a-\omega}^{2n}$, will be $= n^2 \times \omega^2 \times 4a^2 \times \overline{\omega^2 + c^2} \times \overline{\omega^2 + d^2}$, &c. where the factor $4a^2$ takes place instead of $\omega^2 +$ sq. of the tang. of 90°.

If y be $= 1$, and n an odd number, p will be $= 1$, and $b = 0$; but no one of the cosines q, r, s, &c. will be $= -1$, as when n is an even number. Therefore, in this case, the equation $\overline{a+\omega}^{2n} - 2y \times \overline{a+\omega}^{n} \times \overline{a-\omega}^{n} + \overline{a-\omega}^{2n} = \overline{2+2y} \times \overline{\omega^2 + b^2} \times \overline{\omega^2 + c^2}$, &c. becomes $\overline{a+\omega}^{2n} - 2 \times \overline{a+\omega}^{n} \times \overline{a-\omega}^{n} + \overline{a-\omega}^{n} = 4 \times \omega^2 \times \overline{\omega^2 + c^2} \times \overline{\omega^2 + d^2}$, &c.

IX.

IX.

By taking the ſquare root of $\overline{a+\omega}^{2n} - 2 \times \overline{a+\omega}^{n} \times \overline{a-\omega}^{n} + \overline{a-\omega}^{2n}$, and of its two values juſt now found, we have, when n is an even number, $\overline{a+\omega}^{n} - \overline{a-\omega}^{n} = 2\,a\,n\,\omega \times \sqrt{\omega^2 + c^2} \times \sqrt{\omega^2 + d^2}$, &c. $2\,a$ taking place inſtead of $\sqrt{\omega^2 +}$ ſq. of the tang. of 90°.

And, when n is an odd number, $\overline{a+\omega}^{n} - \overline{a-\omega}^{n} = 2\,\omega \times \sqrt{\omega^2 + c^2} \times \sqrt{\omega^2 + d^2}$, &c. Whence the following conſtruction is inferr'd.

X.

Deſcribe about the centre C (*Plate* XX. *fig.* 1. and 2.), with the radius a, the circle $P\,A'\,A''\,A'''$, &c.; draw the diameter $P\,C\,Q$, and the tangent $B'''\,P\,B^5$; divide the ſemicircumference $P\,A'\,Q$ into as many equal parts $P\,A'$, $A'\,A''$, $A''\,A'''$, &c. as there are units in the integer n; draw the ſecants $C\,A'\,B'$, $C\,A''\,B''$, &c. and, taking on $C\,Q$ any point O, draw $K'''\,O\,K^5$ parallel to $B'''\,P\,B^5$; likewiſe draw $B'\,K'$, $B''\,K''$, $B'''\,K'''$, &c. parallel to $P\,Q$; and call $C\,O$, ω.

Then will q be the coſine of twice the angle $P\,C\,A'$, r the coſine of twice $P\,C\,A''$, s the coſine of twice $P\,C\,A'''$, &c. if the radius be 1.

Therefore $P\,B' = O\,K'$ will be $= c$, $P\,B'' = O\,K'' = d$, &c. and $C\,K' = \sqrt{\omega^2 + c^2}$, $C\,K'' = \sqrt{\omega^2 + d^2}$, &c. Conſequently $O\,P^n - O\,Q^n$ being $= \overline{a+\omega}^{n}$

—

$-\overline{\overline{a-\omega}}^{n}$, and $n \times PQ \times CO \times CK' \times CK''$, &c. $= 2\,a\,n\,\omega \times \sqrt{\omega^2 + c^2} \times \sqrt{\omega^2 + d^2}$, &c. when n is an even number; $OP^n - OQ^n$ will then be $= n \times PQ \times CO \times CK' \times CK''$, &c. where the diameter PQ takes place inſtead of the infinite quantity $CK^{\frac{n}{2}}$.

But if n be an odd number, $OP^n - OQ^n$ will be $= 2 \times CO \times CK' \times CK'' \times CK'''$, &c.

XI.

It is evident that, of the factors CK', CK'', CK''', &c. the firſt and laſt, the ſecond and laſt but one, &c. are reſpectively equal to each other. Therefore, omitting the ſquares of the factors below PQ, and the ſquares of their values,

$OP^n - OQ^n$ is $= n \times PQ \times CO \times CK'^2 \times CK''^2 \times CK'''^2$, &c. and $\overline{\overline{a+\omega}}^{n} - \overline{\overline{a-\omega}}^{n} = 2\,a\,n\,\omega \times \sqrt{\omega^2 + c^2} \times \sqrt{\omega^2 + d^2}$, &c. when n is an even number; or

$OP^n - OQ^n$ is $= 2 \times CO \times CK'^2 \times CK''^2 \times CK'''^2$, &c. and $\overline{\overline{a+\omega}}^{n} - \overline{\overline{a-\omega}}^{n} = 2\,\omega \times \sqrt{\omega^2 + c^2} \times \sqrt{\omega^2 + d^2}$, &c. when n is an odd number.

XII.

If we ſuppoſe $y = -1$, and n an odd number, it will appear, by proceeding much in the ſame manner as in *Art.* VIII. that $\overline{\overline{a+\omega}}^{2n} + 2 \times \overline{\overline{a+\omega}}^{n} \times \overline{\overline{a-\omega}}^{n} + \overline{\overline{a-\omega}}^{2n}$ is $= n^2 \times 4a^2 \times \omega^2 + b^2 \times \omega^2 + c^2 \times \omega^2 + d^2$, &c.

&c. where the factor $4a^2$ takes place inſtead of ω^2 + ſq. of the tang. of 90°.

If y be $= -1$, and n an even number, $\overline{a+\omega}^{2n} + 2 \times \overline{a+\omega}^{n} \times \overline{a-\omega}^{n} + \overline{a-\omega}^{2n}$ is $= 4 \times \overline{\omega^2+b^2} \times \overline{\omega^2+c^2}$, &c.

Whence, by extracting the ſquare root of both ſides of thoſe equations, we have, when n is an odd number, $\overline{a+\omega}^{n} + \overline{a-\omega}^{n} = 2an \times \sqrt{\omega^2+b^2} \times \sqrt{\omega^2+c^2}$, &c. $2a$ taking place inſtead of $\sqrt{\omega^2+}$ ſq. of the tang. of 90°: And, when n is an even number, $\overline{a+\omega}^{n} + \overline{a-\omega}^{n} = 2 \times \sqrt{\omega^2+b^2} \times \sqrt{\omega^2+c^2}$, &c. Hence we infer this conſtruction.

XIII.

Having deſcrib'd about the centre C (*fig.* 3. and 4.), with the radius a, the circle $P\,a'\,A'\,a''\,A''$, &c. draw the diameter $P\,C\,Q$, and the tangent $b''\,P\,b^4$; divide the ſemicircumference $P\,a'\,Q$ into as many equal parts $P\,a'$, $a'\,A'$, $A'\,a''$, &c. as there are units in $2n$; draw the ſecants $C\,a'\,b'$, $C\,a''\,b''$, &c. and, thro' any point (O) in $C\,Q$, draw $k''\,O\,k^4$ parallel to $b''\,P\,b^4$; likewiſe draw $b'\,k'$, $b''\,k''$, &c. parallel to $P\,Q$; and call $C\,O$, ω.

Then, if the radius be 1, p will be the coſine of twice the angſe $P\,C\,a'$, q the coſine of twice $P\,C\,a''$, &c. therefore $P\,b' = O\,k'$ will be $= b$, $P\,b'' = O\,k'' = c$, &c. and $C\,k' = \sqrt{\omega^2+b^2}$, $C\,k'' = \sqrt{\omega^2+c^2}$, &c.

Con-

Consequently $O P^n + O Q^n$ being $= \overline{a+\omega}^n + \overline{a-\omega}^n$, and $n \times PQ \times Ck' \times Ck''$, &c. $= 2an \times \sqrt{\omega^2+b^2} \times \sqrt{\omega^2+c^2}$, &c. when n is an odd number; $O P^n + O Q^n$ will then be $= n \times PQ \times Ck' \times Ck''$, &c. where the diameter PQ takes place instead of the infinite quantity $C\overline{\frac{n+1}{2}}$.

But if n be an even number, $O P^n + O Q^n$ will be $= 2 \times Ck' \times Ck''$, &c.

XIV.

It is obvious that, of the factors Ck', Ck'', &c. the first and last, the second and last but one, &c. are respectively equal to each other: Therefore the squares of the factors below PQ, and the squares of their values, being omitted,

$O P^n + O Q^n$ is $= n \times PQ \times Ck'^2 \times Ck''^2$, &c. and

$\overline{a+\omega}^n + \overline{a-\omega}^n = 2an \times \overline{\omega^2+b^2} \times \overline{\omega^2+c^2}$, &c. when n is an odd number; or

$O P^n + O Q^n$ is $= 2 \times Ck'^2 \times Ck''^2$, &c. and

$\overline{a+\omega}^n + \overline{a-\omega}^n = 2 \times \overline{\omega^2+b^2} \times \overline{\omega^2+c^2}$, &c. when n is an even number.

XV.

Writing in the equation $\overline{a+\omega}^{2n} - 2y \times \overline{a+\omega} \times \overline{a-\omega}^n + \overline{a-\omega}^{2n} = \overline{2 \mp 2y} \times \overline{\omega^2+b^2} \times \overline{\omega^2+c^2}$, &c.

(found by *Art.* VII.) $a - u$ for ω, the ſame becomes $\overline{2a - u - 2y}^{2n} u^n \times \overline{2a - u + u^2}^n = \overline{2 \mp 2y} \times \overline{u^2 - 2au + a^2 + b^2} \times \overline{u^2 - 2au + a^2 + c^2}$, &c. $= \overline{2 \mp 2y} \times \overline{u^2 - 2au + \beta^2} \times \overline{u^2 - 2au + \gamma^2} \times \overline{u^2 - 2au + \delta^2}$, &c. if inſtead of $\sqrt{a^2 + b^2}$, $\sqrt{a^2 + c^2}$, &c. (the ſecants of the arcs of which b, c, d, &c. are tangents), we put β, γ, δ, &c.

And, by a like ſubſtitution in the equations in *Art.* XI. and XIV. it appears, that $\overline{2a - u - u}^n$ is $= 2an \times \overline{a - u} \times \overline{u^2 - 2au + \gamma^2} \times \overline{u^2 - 2au + \delta^2}$, &c. or $2 \times \overline{a - u} \times \overline{u^2 - 2au + \gamma^2} \times \overline{u^2 - 2au + \delta^2}$, &c. according as n is an even or an odd number: And that $\overline{2a - u + u}^n$ is $= 2an \times \overline{u^2 - 2au + \beta^2} \times \overline{n^2 - 2au + \gamma^2}$, &c. or $2 \times \overline{u^2 - 2au + \beta^2} \times \overline{u^2 - 2au + \gamma^2}$, &c. according as n is an odd or an even number.

From what is done above, I might now deduce many corollaries; and, by means of other ſubſtitutions, inveſtigate other theorems; but want of leiſure obliges me to deſiſt.

LXXVII. *An*

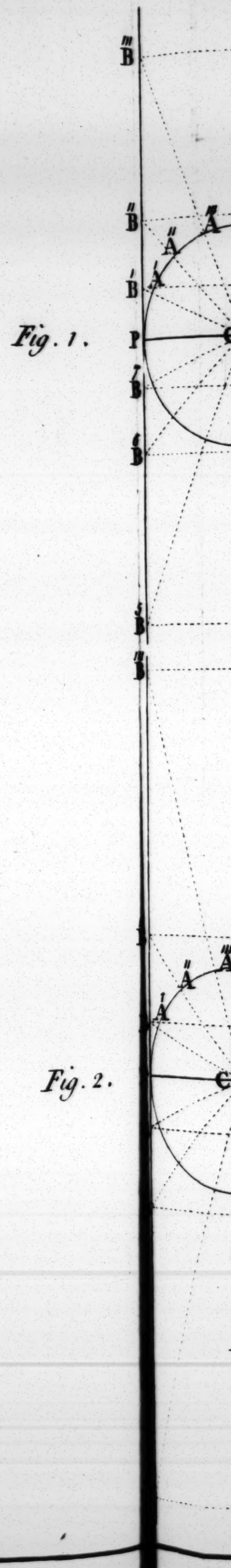


Fig. 1.

Fig. 2.

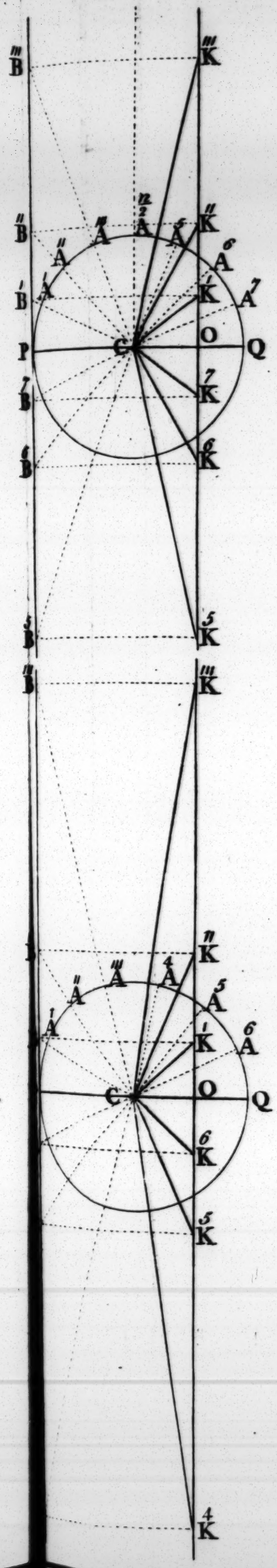


Fig. 3.

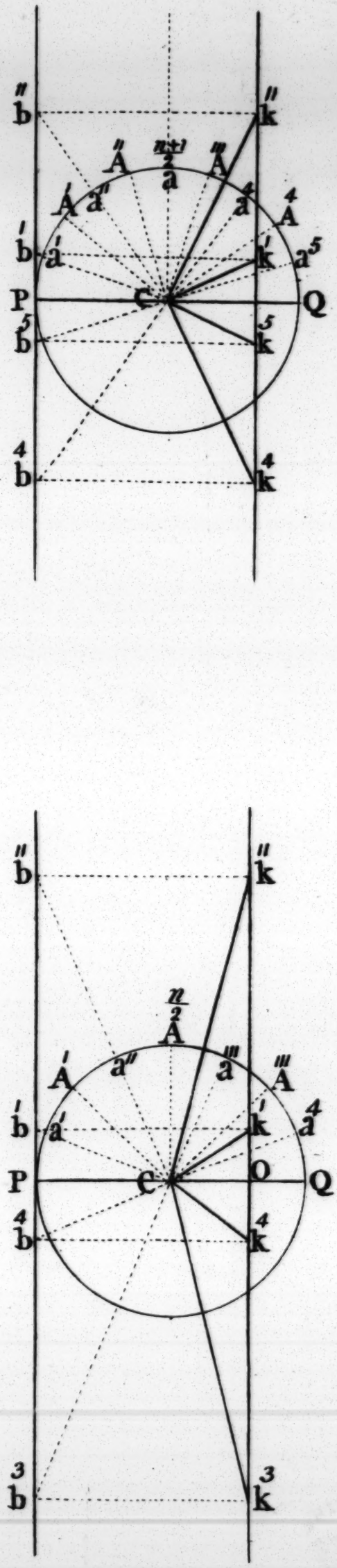


Fig. 4.

J. Mynde sc.

Belgrave in Wedgw. Centenace. 1. Bel p 54.

LXXVII. *An Account of an extraordinary Diſeaſe of the Skin, and its Cure. Extracted from the* Italian *of* Carlo Cruſio; *accompanied with a Letter of the Abbé* Nollet, *F. R. S. to Mr.* William Watſon, *F. R. S. by* Robert Watſon, *M. D. F. R. S.*

Read May 16, 1754.

THE diſeaſe, which lately befel a young Neapolitan woman, being of an extraordinary nature, greatly excited the curioſity of the governors of the royal hoſpital at Naples. Theſe gentlemen engaged Signor Cruſio, the phyſician of that hoſpital, to whoſe care this patient was committed, to draw up a faithful relation of the caſe. The Abbé Nollet, of the Royal Academy of Sciences at Paris, being deſirous of having an authentic account of what he had heard had been ſo much the ſubject of diſcourſe at Naples, procur'd by his friends from Signor Cruſio a tranſcript of this relation; and, believing it not unworthy of attention, tranſmitted it to London. In a letter to Mr. William Watſon, the Abbé Nollet aſſures him, that the relation contains nothing but what is true; "becauſe, ſays he, I have been in-"form'd of the fact by diſintereſted perſons, and be-"cauſe I know the writer of it to be a man of too "much honour to be capable of attempting to im-"poſe upon any one."

The hiſtory of the diſeaſe is as follows.

A Young woman, ſeventeen years old, call'd Patrizia Galiera, the daughter of a citizen of Naples, was brought to the royal hoſpital the twenty-ſecond of June, 1752. and was placed in one of the wards aſſigned to the care of Dr. Cruſio; who viſiting her in her bed, and aſking her the neceſſary queſtions to form a right notion of her diſeaſe, was inform'd by her, that her complaint was an exceſſive tenſion and hardneſs of her ſkin over all her body, by which ſhe found herſelf ſo bound and ſtraiten'd, that ſhe could hardly move her limbs. Upon examining her, he found her ſkin hard to the touch, like wood, or a dry hide; however, he obſerv'd ſome difference in the degrees of the hardneſs: For in ſome places it was greater; as in the neck, forehead, and particularly in the eye-lids; inſomuch that ſhe could neither raiſe nor intirely ſhut them. It was alſo very great in the lips, tongue, and on each ſide of her body; but the muſcles under the ſkin ſeem'd not to be affected, becauſe the joints could be bent; and if in any place there was any difficulty in moving the limbs, this aroſe not from any defect in the muſcles, but from the hardneſs and tenſion of the ſkin and cellular membrane, which did not yield to their contraction and relaxation. For example; ſhe could ſcarce open her mouth; which happen'd not from any fault in the digaſtric, or other muſcles, but from the hardneſs of the ſkin, that cover'd the lips and cheeks, and that would not permit her to draw down the lower jaw. In the ſame manner was ſhe incapable of bending her neck, or turning her head: Neither did this happen from any defect in the

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muſcles deſtin'd to that office, but from the firmneſs of the ſkin and membrane, which in no wiſe yielded to their contraction. This was the caſe in the other parts of the body; the muſcles being, as it were, tied down and compreſſed by a dry, hard, and unpliable covering.

As to other particulars; her ſkin had loſt its natural warmth, but was ſenſible, when it was preſs'd upon by the nails, or a pin; the patient then ſaying, that ſhe felt a pain, as if the ſkin were tearing. Her pulſe was perceiv'd to be deep, and obſcure; but equal, and regular. Her reſpiration was free, and uninterrupted; her digeſtion was good, and ſhe found no inconvenience after eating, except a greater ſtreightneſs, and an uneaſy conſtriction round the belly. As to the natural excretions, the alvine were eaſy and proper; but the urinary ſometimes exceeded the quantity of what ſhe drank, and appear'd loaded with ſalts; both which circumſtances, perhaps, proceeded from the ſenſible and inſenſible perſpiration being intirely wanting. For upon her being aſk'd, whether ſhe ever ſweated, ſhe anſwer'd, that ſhe did not, tho' ſhe was ever ſo much exercis'd and fatigu'd. Her ſleep was natural; ſhe had never had the menſtrual evacuation. She ſaid, her diſorder began firſt in her neck, which ſhe perceiv'd ſhe could not move as uſual; then ſhe found the ſkin of her face and forehead to grow hard; and ſo ſucceſſively, from day to day, ſhe ſaw and felt all the external parts of her body grow hard, and tenſe. She never had had any other diſeaſe except a little fever ſome years before, nor had ever been ſuddenly or exceſſively frighten'd.

So extraordinary a diſorder did not a little diſconcert the gentleman, to whoſe care the cure of it was committed. He judged, that it would be very difficult, and almoſt impoſſible, for him to reſtore the patient to a perfect ſtate of health, not only on account of the uncommon nature of the complaint, but becauſe he could not learn, from the relation of the ſick, any proximate or remote cauſe of the diſeaſe, by which he might be directed to the application of a proper and efficacious remedy. For tho', on firſt conſidering the caſe, the want of the menſtrual diſcharge might appear to have been the occaſion of it; yet as there are many women, who live in perfect health, and conceive, without ever having had this evacuation; the want of it, in this caſe, could not be ſuppos'd an adequate cauſe; and this the event afterwards ſhew'd, ſince the patient was cur'd without ever having had the natural diſcharges of the ſex.

The indication then of cure was to be taken from the preſent ſtate of the diſeaſe. The ſkin was obſerv'd to have loſt its natural ſoftneſs and flexibility; was become hard, contracted, and imperſpirable. Wherefore it was not unreaſonable to conclude, that the immediate cauſe of ſuch a morbid change was a preternatural contraction of the nervous or fibrous parts of the ſkin, by which its excretory ducts and exhaling veſſels were conſtringed, and did not ſupply a due quantity of the oily and aqueous fluids neceſſary to ſoften and lubricate the parts. Now for want of theſe fluids, the coriaceous fibres of the ſkin, the nervous *papillæ*, the *corpus mucoſum*, the abſorbing and exhaling veſſels, and the cuticle, could not but collapſe, coaleſce, grow rigid, dry, firm, and hard:

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And certainly leather, which, in its hardeſt and drieſt ſtate, bears a near reſemblance to the diſeas'd ſkin of this young woman, does not loſe its original ſoftneſs; but either by being ſimply depriv'd of thoſe juices, which moiſten'd its component parts, or elſe by the coagulation and inſpiſſation of ſome of the fluids lodged in the veſſels; which laſt circumſtance perhaps likewiſe contributed its ſhare to the hardneſs of the ſkin in the preſent caſe.

On theſe conſiderations it was thought fit to put the patient into a bath of warm milk and water, and to direct her to ſtay in it a conſiderable ſpace of time, that the warmth and moiſture might relax and ſoften the hardneſs of her ſkin: But ſhe could not bear to continue in the bath, on account of the great oppreſſion and anxiety, which it occaſion'd, and becauſe the troubleſome conſtriction of her ſkin was much increas'd by it. She was therefore put to bed, and well cover'd with cloaths, in hopes to promote a ſweat; but all was in vain; for her ſkin remain'd as hard and dry as before. However, this treatment was repeated for ſix days; but, on going into the bath for the ſeventh time, ſhe was ſeiz'd with convulſions in the muſcles of her legs and arms. This was very unexpected, and made it neceſſary to diſcontinue this method of cure. But as it was imagin'd, that it was the weight and preſſure of the water, which gave her ſo much uneaſineſs, a method was thought on, to avoid this inconvenience, and, at the ſame time, to procure for the patient the benefit, that might ariſe from the relaxation and ſoftening of the ſkin and pores by the abſorption of an external humidity, which was judged to be neceſſary to the cure. Now

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the vapour of warm water hath a great power of inſinuating itſelf into the pores, and between the fibres, of bodies; and, by that means, of relaxing and ſoftening the hardeſt ſubſtances, as is obſerv'd in dry leather; which, ſuſpended in the ſteam of boiling water, becomes much more ſoft and pliable, than if it had been immers'd for a longer time in the hot water itſelf. A vapour bath was therefore order'd, and contriv'd in ſuch a manner, that the ſteam of the boiling water might intirely ſurround the body of the patient, or be directed to any particular part, as occaſion ſhould require. She bore the vapour without any inconvenience, and was conſtantly kept in bed in the intervals between the ſeveral applications of it. The ſixth time of uſing this kind of bath, ſhe began to perſpire a little, and, from day to day, the perſpiration grew more general, and at laſt univerſal: Then the ſkin began to be leſs rough, but not leſs hard; and the urine was more thin and diluted than before. Her diet was preſcrib'd to be of the moſt ſoft and relaxing nature, and principally conſiſted of whey. As ſhe was judg'd to be of too full a habit, and as ſhe had not the regular menſtrual diſcharge, ſhe was order'd to loſe twelve ounces of blood from the foot; and it was thought, that this evacuation might contribute to produce a general relaxation, and, by conſequence, make the circulation of the blood, and other fluids, more free and eaſy through their reſpective canals. It was ſurpriſing to ſee what difficulty the ſurgeon found in opening the vein, on account of the hardneſs of the ſkin; inſomuch that, in the operation, the lancet yielded, and bent. However, at laſt, it pierced the ſkin, and the vein, but not without a

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good deal of pain to the patient. The blood iſſu'd forth with great impetuoſity, and the wound was ſome time before it heal'd; but, at length, it form'd an elevated and hard ſcar.

By continuing the emollient diet and vapour bath, in about forty days the ſkin of her legs began to grow ſoft; in which part, according to the relation of the patient, the hardneſs laſt ſhew'd itſelf. But as often as ſhe expos'd herſelf to the freſh and cool air, the ſkin, which had begun to grow ſoft and flexible, was obſerv'd to grow again hard, and imperſpirable. It was therefore thought proper, towards the end of September, to place her in a warm room, where the air was kept of an equal degree of heat. This had the deſir'd effect: For by ſtaying in her room, and from time to time repeating the vapour bath, and by drinking, at her meals, a decoction of the woods, the perſpiration was conſtant and moderate; and the ſoftneſs of the ſkin, which began in the legs, extended itſelf upwards, and was in ſome degree perceptible in the arms.

Five months were now elaps'd ſince the beginning of this treatment, when it was believ'd, that, without ſome more efficacious medicine, capable, by its motion, weight, figure, and diviſibility, of circulating with the blood, and of penetrating into the moſt remote and ſubtil receſſes of the veſſels, it would be impoſſible to reſolve and open the obſtructions, which were form'd in the vaſcular ſtructure of the ſkin, and which, by hindering the fluids from circulating thro' their reſpective canals, had depriv'd them of that humidity, which nature hath made neceſſary for their flexibility and ſoftneſs. It was therefore thought pro-

per to make her take ſmall doſes of pure quickſilver; and that the mercury might the more eaſily be determin'd to the ſkin, the patient was order'd to be conſtantly kept in a warm air, to have the ſurface of her body rubb'd with a flannel, and to continue the uſe of the vapour bath. But, by way of preparation for this mercurial courſe, ſhe was gently purged, and blooded a ſecond time, that the plenitude being diminiſh'd, the mercury might better circulate through the fineſt veſſels. Here it is to be obſerv'd, that the ſurgeon, in this ſecond blood-letting, did not meet with that reſiſtance, in piercing the ſkin, which he had experienced in the firſt. The patient, thus prepar'd, began in December, 1752. to take daily ſix, and afterwards twelve, grains of pure quickſilver, in a drachm of caſſia, drinking after it half a pint of a decoction of ſarſaparilla. In this courſe ſhe continu'd four months with chearfulneſs, and without any inconvenience; and within two months from the beginning of it there appear'd a ſomewhat viſcid ſweat, and the ſkin grew more flexible, and yielding. About the end of March, 1753. ſhe had an efflorеſcence over all her ſkin, which, by degrees, became puſtular, and was very troubleſome by its heat and itching. The uſe of mercury was then diſcontinu'd, and ſhe took no medicine but half a pint of an infuſion of ſarſaparilla in the morning, and an emulſion of melon and poppy-ſeeds in the evening. Then the heat and itching abated, and the puſtules ſuppurated. Signor Cruſio ſays, that he had the pleaſure to ſee many ſmall globules or particles of mercury ſeparated in the ripe puſtules. This is ſomething ſo unuſual and ſurpriſing, that we ſhall ſcarce be inclined to give

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our aſſent till we are forced to it by farther experience and obſervation; eſpecially as we know, that the moſt careful and ſenſible men are frequently miſtaken; but that it is very rare, that any thing happens out of the ordinary courſe of nature.

About the middle of May following, her ſkin was quite clear of puſtules, and was become perfectly ſoft and flexible, being capable of being mov'd, rais'd, extended, and of performing all its natural functions. This ſoftneſs and flexibility of the ſkin was general, except in the forehead and lips; which, however, afterwards recover'd their natural ſtate.

But there ſtill remains an unuſual degree of tenſion in ſome of the muſcles, which lie immediately under the ſkin, particularly in thoſe of the hand and radius; on which account, a milk diet is preſcrib'd, to ſupply the blood with a proper matter for filling the cells of the adipoſe membrane; which membrane, by having ſuſtain'd a long preſſure between the diſeas'd ſkin and muſcles, is become depriv'd of its proper mucilaginous and oily juices, deſign'd by nature to keep the parts ſoft and flexible, and to facilitate the motion of the muſcles. Now when, by a fit diet, the oily and mucilaginous particles ſhall again abound in the blood, they will, ſince the morbid preſſure of the ſkin is remov'd, be depoſited in their proper cells, and, by that means, it is hop'd, that the affected muſcles, which every day grow leſs tenſe, will ſoon be reſtor'd to their natural ſtate; and that the cure will be as complete, with reſpect to them, as it already is with regard to the ſkin.

Belgice in Verhandelingen. D. Deel p. 160.

LXXVIII. *Experiments concerning the Uſe of the Agaric of Oak in ſtopping of Hæmorrhages.*

I.

An Account of the Event of Experiments made by Agaric upon the Amputation of the legs of two Women in Guy's *Hoſpital. By Mr.* Samuel Sharp, *Surgeon of that Hoſpital, and F. R. S.*

Dec. 12, 1752.

Read Dec. 14, 1752. PUrſuant to a recommendation from the chair on Thurſday the ſeventh inſtant, the agaric was made uſe of, the enſuing Saturday, on two women, whoſe legs were amputated below the knee.

One of them was ſixty-two years of age, and had been very much impair'd by a long illneſs, and continual pain. During the operation, ſhe bled with great impetuoſity; and it was with difficulty, that the hæmorrhage was ſtopp'd, notwithſtanding I preſs'd the agaric, with all my force, againſt the extremities of the *tibialis antica*, and *tibialis poſtica*, the two largeſt arteries. The tendency to bleed, after the operation, was ſuch, that I found it neceſſary to apply the tourniquet, and keep a tight ſtricture on the femoral artery. She complain'd grievouſly of the pain ariſing from the ſtricture; upon which it was a little looſen'd, and ſoon after a hæmorrhage enſu'd from one of the two large arteries, which was immediately

ately taken up, and tied with a needle and ligature. In order to diſcover this veſſel, the agaric was remov'd; and tho' the tourniquet was quite ſlack, the other large artery did not bleed one drop. This happened about an hour and an half after the operation. After the veſſel was tied, the ſame agaric was again laid on the ſame part, without ſcrewing the tourniquet, and the patient became much eaſier; but, in about three hours and an half, the other large veſſel burſt open; and tho' aſſiſtance was on the ſpot, and it was immediately tied up, ſhe was ſo exhauſted by the ſudden loſs of blood, that ſhe died in about twenty minutes.

It is conjectured, that, by the three hæmorrhages, *viz.* the firſt during the operation, and the two after the operation, ſhe loſt between twenty and thirty ounces of blood. I examined the limb after death, but found no ſingular appearance in the veſſels, or the adjacent parts.

The other woman was twenty-four years of age. She loſt very little blood in the operation, and has continued extremely well ever ſince. The agaric ſeems, in this inſtance, to have anſwered the moſt ſanguine expectations. The dreſſings were removed this morning, without any accident. Mr. Warner, who performed the amputation, propoſes to do himſelf the honour of laying the particulars of her caſe before the Society.

Samuel Sharp.

II. *The*

II.

The Hiſtory of a Caſe relating to the Effects of the Agaric of Oak in ſtopping of Hæmorrhages. By Joſeph Warner, *Surgeon to* Guy's *Hoſpital, and F. R. S.*

Hatton-Garden,
Dec. 14, 1752.

Gentlemen,

Read Dec. 14, 1752. AGreeably to the deſire of this Society, I have embraced the firſt opportunity of communicating to you the effects of the agaric of the oak in ſtopping of hæmorrhages, or the bleeding from principal veſſels after amputation. If I have been too circumſtantial in my narrative of the ſymptoms attending the following caſe, I hope you will attribute it merely to the deſire I have of giving you all the ſatisfaction I am capable of, relating to the experiment of this ſtyptic: And this I have been more particularly induced to, as it is one of the firſt caſes of the kind, offered to your conſideration, where this application has been made uſe of in England.

Saturday December 9, 1752. Catharine Spong, aged twenty-four, had her leg amputated, about four inches below the knee, at twelve o'clock to-day, on account of an incurable ulcer, with which ſhe had been afflicted for thirteen years. She loſt very little blood by the operation. Immediately after the amputation, a piece of agaric, of a proper ſize (the ſame which was deliver'd to Mr. Sharp by the Royal Society for this purpoſe), was applied to the mouths of the principal arteries. Two other pieces of agaric were applied to the mouths of two ſmaller arteries, which appeared at ſome diſtance

ſtance from the principal ones. Upon the pieces of agaric, doſſils of lint were applied, and over all a pledgit of tow ſpread with the common digeſtive; all which were kept on by the common bandages made uſe of in the like caſes, and applied with the ſame degree of tightneſs as uſual.

For an hour and a quarter after the operation, the ligature and tourniquet were kept on, moderately tight, at a convenient diſtance above the knee, at the end of which time it was ſlackened ſo as to have no degree of preſſure upon the femoral artery, as the dreſſings and rollers appeared very little tinged with blood. The patient was much eaſier than I had ever obſerved, after the uſe of the needle and ligatures. Her pulſe appeared very little diſturbed till about four o'clock this afternoon, when the ſymptomatic fever began to come on, attended now and then with convulſive twitches of the ſtump, and thigh; for which reaſons, the ligature was ſomewhat tightened. At ſeven o'clock this evening the ligature and tourniquet were quite looſened; ſoon after which, the convulſive twitches became leſs frequent, and leſs ſevere. Theſe convulſive twitches ſhe had been long uſed to, and, by her own account, they were more ſevere before the operation, than they have been ſince. She had but little reſt to-night.

Sunday morning, at half an hour after ten o'clock, ſhe appeared as well as could be expected, her pulſe was calm, and ſhe had no particular complaints. At twelve o'clock at night ſhe fell aſleep, and ſo continued till after ſeven o'clock the next morning.

Monday

Monday morning ſhe appeared well, her pulſe was calm, and ſhe had no particular pain. Monday night ſhe ſlept but little, but was very eaſy the whole night.

Tueſday morning ſhe appeared well, her pulſe quiet. This morning, at eleven o'clock, ſhe was dreſs'd in the uſual manner: The wound appear'd with a very good aſpect; ſhe has ſuffer'd no particular pain in the parts where the agaric was apply'd, and is, in all reſpects, as well as can be expected. At ſeven o'clock this evening I viſited her again: She was perfectly eaſy; the convulſive twitches, which ſhe at firſt complained of, are quite removed, and have been ſo ever ſince yeſterday morning.

Wedneſday morning, ſhe continues well, and perfectly eaſy; has had no return of her convulſive twitches, nor is there any appearance of blood thro' the rollers, or dreſſings.

Thurſday, Dec. 14. ſhe continues very well. Her wound was dreſſed this morning, at half an hour after eleven o'clock, when there appeared a very proper diſcharge of matter, not in the leaſt tinged with blood. The whole of the agaric, with the reſt of the dreſſings, are come off, without giving pain. She had yeſterday two or three convulſive twitches of the ſtump, and thigh, but they were ſlight. Her pulſe is good.

III. *An*

III.

A ſhort Hiſtory of the Effects of the Agaric of the Oak in ſtopping of Bleedings after ſome of the moſt capital Operations in Surgery; with an Acconnt of the Manner of its acting upon the Veſſels. *By* Joſeph Warner, *F. R. S. and Surgeon to* Guy's *Hoſpital.*

Read May 23, 1754.

IN December 1752. I had the honour of communicating to the Royal Society the good effects of the agaric of the oak in the caſe of a young woman, aged twenty-four years, whoſe leg I had cut off below the knee. Since that time, I have not heard of any farther trials, which have been made with it, nor of any accounts, that have been given to the Society, of its great uſefulneſs in ſurgery. This may probably ariſe from the virtues of the agaric not being, as yet, much known in England; or from the unwillingneſs of ſurgeons to adopt ſuch a method, as they may probably ſuppoſe to be attended with hazard.

The great ſucceſs, which attended my firſt experiment of this kind, was a ſufficient inducement to make a farther trial of it in other caſes of the like nature. This I have done in four more inſtances, all of which have been attended with ſucceſs equal to the firſt. The particular advantage of the application is evidently this; that it has the power of effectually reſtraining the bleeding without giving pain; for which reaſon, there appears to be leſs of

the ſymptomatic fever, than what occurs after the uſe of the needle and ligature, which is, by much, the moſt painful proceſs of the operation in amputations, and is ſometimes productive of fatal convulſions, as has been obſerv'd by Monſieur le Dran, in his chapter of amputations; under which circumſtances, he particularly adviſes the immediate removal of the ligatures as a cure.

The ligatures have ſometimes the farther inconvenience of remaining quite fix'd to the laſt; on which account, they unavoidably retard the healing of the wound.

Beſides the effect of reſtraining the hæmorrhage in all recent wounds, which the agaric has in common with the ligature, it has one great advantage peculiar to itſelf; which is that of effectually reſtraining the bleeding in wounds of ſeveral days or weeks ſtanding, where the parts are become ſo rotten, as to be incapable of bearing the ligature. This I have known the effect of in ſeveral inſtances, where the ligature has been attempted in vain.

The manner, in which the agaric acts, is, by contracting, or purſing up the extremities of the divided veſſels. I had an opportunity of enquiring into the fact, in the caſe of a patient, whoſe leg was cut off below the knee. Immediately after the amputation, pieces of agaric were apply'd to, and properly ſecur'd upon, the mouths of all the principal veſſels. In about an hour after the operation, the ſtump bled afreſh; on which account, I remov'd all the dreſſings, except thoſe pieces of agaric, that were at firſt apply'd. This gave me an opportunity of diſcovering the ſource of the hæmorrhage,

rhage, which was from a collateral veſſel, at leaſt an inch diſtant from the principal ones.

When I had ſecur'd the veſſel, I had the curioſity to remove the bits of agaric from thoſe very parts, to which they were at firſt apply'd. I obſerv'd the mouths of the veſſels to be totally contracted in ſo ſhort a ſpace of time, and to be capable of reſiſting the whole force of the circulation; the extremities of the veſſels being alter'd from their natural ſhape of a cylinder to that of a cone.

After having ſaid thus much in favour of the agaric, I ſhall conclude with a reference to the following caſes, as ſome confirmation of what has been advanced above.

Caſe 1. Jonathan Lee, aged fifty-one, had his leg cut off, below the knee, on the 7th of this inſtant May, 1754. He was extremely reduced, in conſequence of the diſeaſe; and the whole maſs of blood was become ſo much impoveriſh'd, and alter'd from its natural ſtate, as to appear like ſerum, both in texture, and colour. During the operation, the ſcrew-tourniquet was apply'd to the thigh with a degree of tightneſs ſufficient to prevent the courſe of the blood.

Immediately after the amputation, bits of agaric were apply'd to the mouths of the veſſels, and upon them ſoft layers of lint; all of which were cover'd with a pledgit of tow ſpread with digeſtive, and were properly ſecur'd on by the common bandage. About three or four minutes after he was roll'd up, and put to bed, I diſcover'd the blood to diſcharge freely thro' the dreſſings; upon which, I tighten'd the tourniquet, in expectation of ſtopping the bleeding, but it

appear'd evidently to increaſe it. Seeing this uncommon effect, I quite ſlackened the tourniquet; upon which, the bleeding immediately ceaſed. This I was led to from a ſuppoſition, that the veins had probably ſuffered ſo great a compreſſion from the inſtrument, as to be incapable of returning that blood, which was carried to the neighbouring parts by the collateral arteries ariſing from the principal trunk above the ligature. But whether this was the true reaſon, or not, I cannot take upon me to determine: However the fact was, that the bleeding immediately ceaſed, and did not return again.

The patient was dreſſed on the fourth day after the operation, and the whole of the agaric was removed. Since that time he has been treated in the common method, without any farther uſe of the agaric. The patient has had very little fever, or pain, ſince the operation. He has a fair proſpect of doing well.

Caſe 2. Elizabeth Hillier, a very luſty woman, thirty-eight years of age, had her breaſt cut off on the 7th of this inſtant May, 1734. The wound was large, and bled freely from ſeveral conſiderable arteries. I made uſe of no other method to ſtop the bleeding, than the application of pieces of agaric to the mouths of the veſſels, which were properly ſecured on by a flannel roller, after being firſt covered with lint, and a pledgit of tow ſpread with digeſtive. The ſymptomatic fever was very ſlight: She has been quite free from thoſe painful ſpaſms, which conſtantly ariſe from the uſe of the ligature: There has not been the leaſt loſs of blood ſince the operation.

Her

Her wound was dreſſed on the fourth day, when the whole of the agaric came away: Since that time it has been treated in the common method. She is in a very fair way of recovery.

Caſe 3. George Whitmore, aged twelve years, had his leg cut off, below the knee, on the 13th of this inſtant May, 1754. The agaric and dreſſings were applied as in the preceding caſes, which has anſwered perfectly well in all reſpects. The tourniquet was quite removed in ten minutes after the operation; he has had very little fever, reſtleſſneſs, or pain. His wound was dreſſed on the fifth day, and the whole of the agaric was removed. He is as well as can be expected.

Caſe 4. Richard Barnat, aged fifty-four, had his leg cut off, below knee, on the 21ſt of this inſtant May, 1754. I made uſe of no other methods, to ſtop the bleeding, than the agaric, which was applied as in the preceding caſes. Immediately after the operation, the patient was put to bed, and the tourniquet let quite looſe. He has not ſuſtained the leaſt loſs of blood ſince the operation. The pain and fever have been very inconſiderable, and he ſeems to be in a very fair way of doing well.

Remark.

The caſe of Jonathan Lee has ſomething ſingular in it, and particularly proves the extraordinary efficacy and uſefulneſs of the agaric. I have already taken notice, how frequently it is impracticable to make a proper uſe of the needle and ligature, in wounds of long ſtanding; and I farther know, from experience, that it is no uncommon thing to meet with the

the ſame diſappointment, even in recent wounds, that are made upon diſeaſed parts. So that I think it may be reaſonably queſtioned, whether I ſhould not have met with the like difficulty in this ſubject, had I attempted to ſecure the veſſels by ligatures.

Hatton-Garden, Thurſday
May 23, 1754.

LXXIX. *Deſcription of a new Pyrometer, with a Table of Experiments made therewith.* By *Mr.* J. Smeaton, *F. R. S.*

Read May 23, 1754. AS it may tend to illuſtrate the following account, it may not be improper to make mention of the properties, that a complete inſtrument, for meaſuring the expanſions, that metalline bodies are ſubject to by heat and cold, ought to be endowed with.

And, firſt, ſince the quantities of thoſe expanſions muſt be proportionable to the length of the bar to be meaſured; the longer the bar, the more ſenſible the expanſion: And therefore ſuch a conſtruction is beſt, as (*cæteris paribus*) will admit of the longeſt bar.

Secondly, That the ſcale, whereupon thoſe minute alterations are to be meaſured, ought to be, at leaſt, ſo large, that the ſmalleſt change in the length of the bar, which the inſtrument is capable of being with certainty affected with, ought to be perceiveable thereon.

Thirdly,

Thirdly, As the ſame change, with reſpect to the poſition of the index and ſcale, will enſue, upon the ſuppoſition, that the materials, compoſing the inſtrument itſelf, are expanded in a certain degree, and the bar applied to be meaſured remains unchanged in its length; as if the inſtrument were ſuppoſed to ſuffer no expanſion, and the bar to be meaſured were ſuppos'd to expand in the former degree: It is therefore neceſſary, that, in the making uſe of an inſtrument of this kind, the materials, of which thoſe parts are compoſed, upon which the meaſure depends, and which may be called the baſis thereof, ſhould be ſubject to no expanſion or contraction during ſuch trial, or that the expanſion or contraction thereof ſhould be capable of being known, and accounted for.

Fourthly, That as all bodies grow ſtill longer by the application of a greater degree of heat; to compare the expanſions of different bodies, we ought to have ſome method of heating them in the ſame degree, notwithſtanding their difference of texture, ſpecific gravity, *&c.*

Fifthly, The ſeveral parts, upon which the meaſurements depend, ought to be ſufficiently large, to be themſelves actually meaſured; that not only the proportions of increaſe of length in different metals, by the ſame degrees of heat, may be known; but alſo the quantities of thoſe expanſions, in real meaſures: Or, in other words, the proportions, that their increaſe of length, between certain degrees of heat, bears to the length of the bodies: By which means, we are enabled to aſcertain the changes, that bodies undergo in their dimenſions by the application of any given degrees of heat.

With

With reſpect to the firſt property, this inſtrument is capable of receiving a bar 2 feet 4 inches long, and might be made capable of receiving bars of a much greater length, of ſome kinds of materials, but not of others; on account of the flexibility brought upon them by a degree of heat not greater than boiling water.

The meaſures taken by this inſtrument are determined by the contact of a piece of metal with the point of a micrometer-ſcrew. The obſervation is the beſt judg'd of by the hearing, rather than that of the ſight or feeling. By this method I have found it very practicable, to repeat the ſame meaſurement ſeveral times, without differing from itſelf above one twenty-thouſandth part of an inch. This principle of determining meaſures by contact is not wholly new; but has been employ'd on ſeveral occaſions, as I am inform'd, by the late Mr. Graham: But the preſent manner of applying thereof, I believe, is ſo; and the degree of ſenſibility ariſing therefrom exceeds any thing I have met with. As the method will eaſily appear by the draught (ſee *Plate* XXI.), I ſhall avoid a farther deſcription of it in this place *.

As no ſubſtance has hitherto been diſcover'd in nature, that is perfectly free from expanſion by heat, I choſe to conſtruct this inſtrument in ſuch a manner, that

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* I have lately ſeen an inſtrument at Mr. Short's, made by the late Mr. Graham, for meaſuring the minute alterations, in length, of metal bars; which were determin'd by advancing the point of a micrometer-ſcrew, till it ſenſibly ſtopp'd againſt the end of the bar to be meaſur'd. This ſcrew being ſmall, and very lightly hung, was capable of agreement within the 3 or 4000th part of an inch.

that the bar, which makes the baſis of the inſtrument, ſhall in each experiment ſuffer the ſame degree of heat, as the bar to be meaſur'd: Of conſequence, the meaſures taken by the micrometer are the differences of their expanſion. The expanſion then of the baſis between two given degrees of heat being once found, the abſolute expanſion of any other body, by adding or ſubtracting the difference to or from the expanſion of the baſis, according as the body to be meaſur'd expands more or leſs than the baſis, will alſo be determin'd.

When the inſtrument is made uſe of, it is immerg'd, together with the bar to be meaſur'd, in a ciſtern of water; which water, by means of lamps apply'd underneath, is made to receive any intended degree of heat, not greater than that of boiling, and thereby communicates the ſame degree of heat to the inſtrument, the bar, and to a mercurial thermometer immerg'd therein, for the purpoſe of aſcertaining that degree. That this may be truly the caſe, the water ſhould be frequently ſtirr'd, that there may be no difference of heat in the different parts of the water: This being done, the height of the quick-ſilver appearing ſtationary, the contact with the ſcrew of the micrometer alſo remaining the ſame, for a ſpace of time, it is to be ſuppos'd, that the heat of the three bodies will be the ſame, as the heat of the water, however different they may be in ſpecific gravity, &c. The whole difficulty is now reduc'd to this problem, *viz.*

To find the abſolute expanſion of the baſis between any two given degrees of heat, not greater than that of boiling water.

For this purpoſe, let there be prepar'd a bar of ſtrait-grain'd white deal, or cedar; which, it is well known, are much leſs expanſible by heat than any metal hitherto diſcover'd: Let the bar be adapted to the inſtrument in like manner as the other bars intended to be meaſur'd; but that the ſoftneſs of the wood may not hinder the juſtneſs of its bearings, let its ends be guarded with a bit of braſs let into the wood at the points of contact, to prevent, as much as may be, the moiſture or ſteam of the water from affecting the wood; let it firſt be well varniſh'd, and then, being wrapp'd round with coarſe flax from end to end; this will, in a great meaſure, imbibe the vapour, before it arrives at the wood. Let the ciſtern alſo be ſo contriv'd, that the inſtrument being ſupported at a proper height therein, the bar to be meaſur'd may, upon occaſion, be above the cover, while the baſis remains in the water: Thus will the cover alſo be a defence againſt the moiſture. Let the water in the ciſtern be now brought to its lower degree of heat (ſuppoſe at or near the freezing point), the baſis having continu'd long enough in the water to receive the ſame degree of heat, and the wooden bar having been previouſly kept in an adjacent room, not ſubject to ſudden alterations of temperature by fire, or other cauſes; let the bar be apply'd to the inſtrument, and the degrees of the micrometer and the thermometer read off, and ſet down: Let the wooden bar be then reſtor'd to its former place, till the water is heated to the greater degree intended (ſuppoſe at or near that of boiling water); the lid being now ſhut down, and the chinks ſtopp'd with coarſe flax, to prevent the iſſuing of the ſteam as much as poſſible, let the

the wooden bar be again brought forth apply'd to the inſtrument, and the degrees of the micrometer and thermometer read off, as before: The difference of degrees of the micrometer, correſponding to the difference of degrees of the thermometer, will expreſs the expanſion of the baſis between thoſe degrees of heat; that is, upon the ſuppoſition that the wooden bar was of the ſame length, at the time of taking the ſecond meaſure, as at the firſt: Indeed a meaſure can hardly be taken without any loſs of time, as the whole of the inſtrument, when the hot meaſure is to be taken, is conſiderably hotter than the wooden bar; and, in caſe of boiling water, the ſteam being very repellent and active, the bar is liable to be ſenſibly affected in its length, before the meaſure can be taken, both by heat and moiſture, which both tend to expand the bar: But as the quantity is ſmall, and capable of being nearly aſcertain'd, a wooden bar, thus apply'd, will anſwer the ſame end as if it was unalterable by heat or moiſture.

In order, therefore, to know the quantity of this alteration, let the time elaps'd between the firſt approach of the bar to the inſtrument, and the taking of the meaſure, be obſerv'd by a ſecond-watch, or otherwiſe; after another equal interval of time, let a ſecond meaſure be taken; and after a third interval, a third; and a fourth; the three differences of theſe four meaſures will be found nearly to tally with three terms of a geometrical progreſſion, from which the preceding term may be known, and will be the correction; which, if apply'd to the meaſure firſt taken, reduces it to what it would have been if the wooden bar had not expanded during the taking thereof.

From a few obſervations of this kind, carefully repeated, the expanſion of the baſis may be ſettled; and this once done, the making experiments upon other bars will become very eaſy, and compendious.

The baſis of this inſtrument (as well as other parts thereof) is braſs. I choſe this ſubſtance rather than any other whoſe expanſion was greater or leſs, becauſe I found, from ſome groſs experiments previouſly made, that the expanſion of braſs was nearly a medium between thoſe bodies, which differ moſt in their expanſion: A conſiderable convenience ariſes from this circumſtance; becauſe as the meaſures, taken in common experiments, are their difference from braſs, the dependence upon the thermometer will be leſs, as theſe differences are leſs. This precaution I have found the more neceſſary, as the greateſt errors that experiments made with this inſtrument are ſubject to, ſeem to be chiefly owing to the thermometer, tho' that which I us'd was well graduated, and good in other reſpects; but this muſt neceſſarily happen, as the ſcale and ſenſibility of the micrometer, when thoſe metals were try'd which differ moſt from the baſis, were greater than that of the thermometer.

The bar of braſs which compoſes the baſis is an inch broad by half an inch thick, and ſtands edgeways upwards; one end is continu'd of the ſame piece at right angles, to the height of three inches and an half, and makes a firm ſupport for the end of the bar to be experimented; and the other end acts upon the middle of a lever of the ſecond kind, whoſe fulcrum is in the baſis; therefore the motion of the extremity of the lever is double the difference between the expanſion of the bar, and the baſis.

This

This upper part of the lever riſes above the lid of the ciſtern, ſo that it and the micrometer-ſcrew are at all times clear of the water. The top of the lever is furniſh'd with an appendage which I call the *feeler*; it is the extremity of this piece which comes in contact with the micrometer-ſcrew. The conſtruction and application hereof will better appear from the draught, than from many words. It hence appears, that, having the length of the lever from its fulcrum to the point of ſuſpenſion of the feeler, the diſtance between the fulcrum and the point of contact with the bar, the inches and parts that correſpond to a certain number of threads of the micrometer, and the number of diviſions in the circumference of the index-plate; the fraction of an inch expreſs'd by one diviſion of the plate may be deduc'd: Thoſe meaſures are as follows.

	Inches.
From the fulcrum of the lever to the feeler	5.875
From the fulcrum to the plate of contact	2.895
Length of 70 threads of the ſcrew . .	2.455
Diviſions in the circumference of the index-plate 100.	

Hence the value of one diviſion will be the $\frac{10}{17863}$ part of an inch: But if the ſcrew be alter'd $\frac{1}{4}$ of one of theſe diviſions, when the contact between the ſcrew and feeler is well adjuſted, the difference of contact (if I may ſo call it) will be very perceivable to the ſlighteſt obſerver; and, conſequently, $\frac{1}{71450}$ part of an inch is perceivable in this inſtrument.

There is one thing ſtill remains to be ſpoke of, and that is, the verification of the micrometer-ſcrew, which

which is the only part of this inſtrument that requires exactneſs in the execution; and how difficult theſe are to make, perfectly good, is well known to every perſon of experience in theſe matters; that is, that the threads of the ſcrew may not only be equidiſtant, in different places, but that the threads ſhall be equally inclin'd to the axis in every part of the circumference.

As nearly the ſame part of the ſcrew is made uſe of in theſe experiments, the latter circumſtance is what principally needs enquiry. For this purpoſe, let a thin ſlip of ſteel, or other metal, be prepar'd, whoſe thickneſs is about $\frac{1}{8}$ of the diſtance of the threads: Let the edges of this thin plate be cut into ſuch a ſhape, as exactly to fit into the fix'd notch in which one end of the bar is laid: Let a ſcrew paſs thro' the ſtandard of braſs, on which that notch is ſupported, in ſuch a manner, that the end of the bar to be meaſur'd that is fartheſt from the lever, may take its bearing againſt the point (or rather the ſmall hemiſpherical end) of this ſcrew: Let one of the braſs bars, us'd in the other experiments, be apply'd to the inſtrument, and a meaſure taken; then let the thin plate be put in between the end of the bar and the point of the ſcrew laſt mention'd, and again take the meaſure; but firſt obſerve, that the plate is put down to the notch, ſo that the ſame place of the plate may always agree with the point of the ſcrew, and, conſequently, no error may ariſe from a different thickneſs in different places of the plate: Obſerve alſo, that the whole comes to a true bearing; then advance the ſame ſcrew till the micrometer-ſcrew is puſh'd backward $\frac{1}{4}$ of a revolution; again repeat the meaſure with

with and without the thin plate; again advance the former ſcrew, ſo as to make that of the micrometer recede another quarter of a turn, and repeat the meaſures with and without the thin plate. This method being purſu'd as far as neceſſary, it is evident, that, the thickneſs of the plate being always the ſame, if the difference of meaſures, taken with and without it, are not always the ſame in the different parts of a revolution of the micrometer-ſcrew, that this ſcrew is not equiangular; but from the differences of the meaſures correſponding to the thickneſs of the ſame plate, in the different parts of a revolution, the errors thereof may be nearly aſſign'd. For greater certainty in this examination, leſt the heat of the obſerver's body ſhould affect the bar or inſtrument during the obſervation, let the whole be immerg'd in the ciſtern of water, which ought to ſtand a ſufficient time before the obſervation is begun, to acquire the ſame temper as the air, which alſo ought to be in a ſettled ſtate.

In this manner I examin'd ſuch threads of this ſcrew as were made uſe of in the following experiments, but did not find any material errors.

The reſult of the experiments made with this inſtrument agrees very well with the proportions of expanſion of ſeveral metals given by Mr. Ellicott; which were deduced from his pyrometer publiſh'd in the *Philoſophical Tranſactions*: And, conſidering the very different conſtruction of the two inſtruments, they abundantly tend to confirm each other.

References

References to the Figures.

Fig. 1. repreſents the inſtrument independent of the ciſtern in which it is us'd.

A B C D, is the main bar or baſis of the inſtrument.

E F, is the bar to be meaſur'd, lying in two notches; one fix'd to the upright ſtandard *A B*, the other to the principal lever *H I*. The end *E* of the bar *E F*, bears againſt the point of

G, a ſcrew of uſe in examining the micrometer-ſcrew.

The other end of the bar *F* bears againſt a ſmall ſpherically protuberant bit of hard metal fix'd at the ſame height as *G*, in the principal lever *H I*.

K, is an arbor fix'd in the baſis, which receives at each end the points of the ſcrews *H L*, upon which the lever *H L* turns, and ſerve as a fulcrum thereto.

O, is a ſlender ſpring, to keep the lever in a bearing ſtate againſt the bar; and

P, is a check, to prevent the lever from falling forward when the bar is taken out.

N, is the feeler, ſomething in the ſhape of a T, ſuſpended, and moveable up and down upon the points of the ſcrews *I M*, which, as well as *L H*, are ſo adjuſted, as to leave the motion free, but without ſhake.

Q R, is the handle of the feeler, moveable upon a looſe joint at *R*; ſo that, laying hold of it at *Q*, the feeler is mov'd up and down without being affected by the irregular preſſure of the hand.

The extremity *s* of the feeler is alſo furniſh'd with a bit of protuberant hard metal, to render its con-

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Fig. 1. p. 6

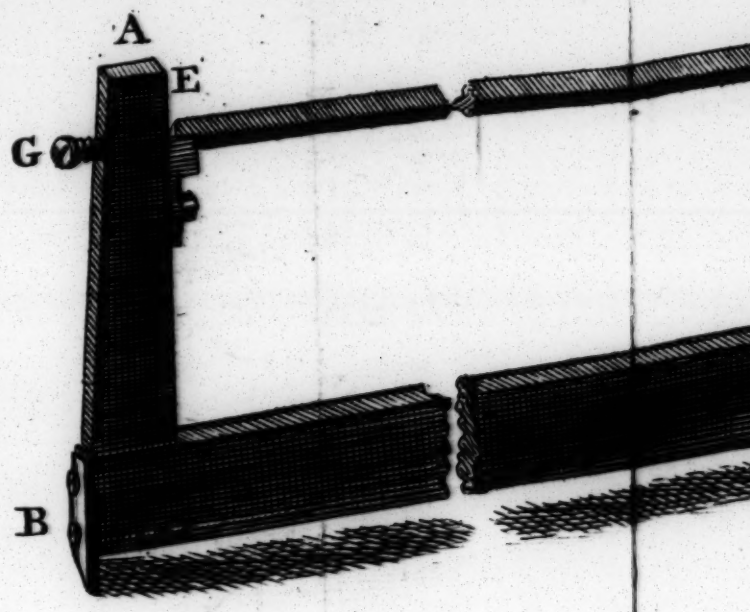
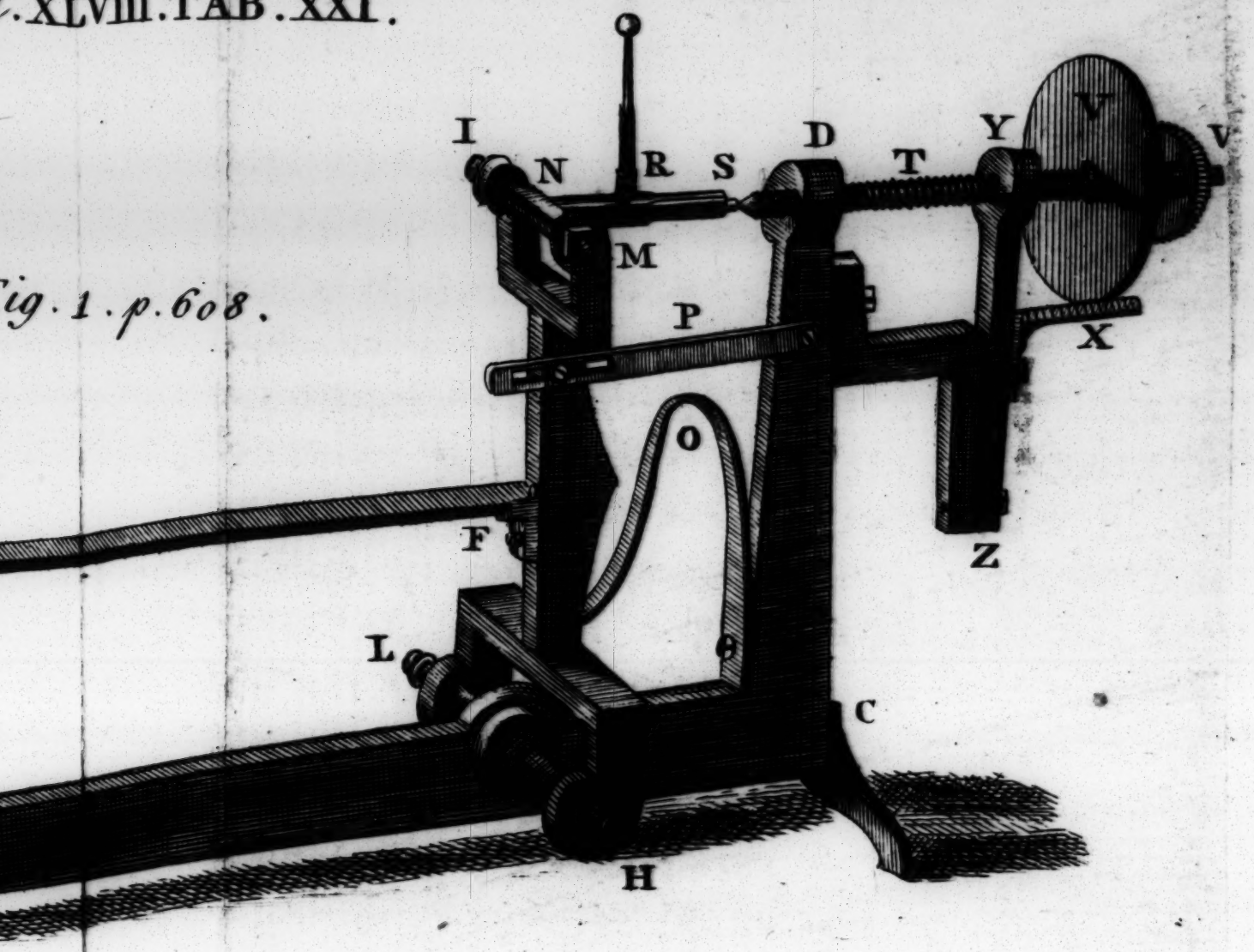


Fig. 2. p. 609.

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ig. 1. p. 608.

. 2. p. 609.

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tact with the point of the micrometer-ſcrew more perfect.

I, is the micrometer-ſcrew; *V*, is the divided index-plate, and *W* a knobb for the handle.

The micrometer-ſcrew paſſes thro' two ſolid ſcrew'd holes at *D* and *Y*.

The piece *Y Z* is made a little ſpringy, and endeavours to pull the ſcrew backwards from the hole at *D*; of conſequence keeps the micrometer-ſcrew conſtantly bearing againſt its threads the ſame way, and thereby renders the motion thereof perfectly ſteady and gentle.

X, is the index, having diviſions upon it, anſwering to the turns of the ſcrew. This piece points out the diviſions of the plate, as the face of the plate points out the diviſors upon the index.

When the inſtrument is us'd; lay hold of the knob at *Q*, with one hand, and, moving the feeler up and down, with the other move forward the ſcrew *I*, till its point comes in contact with the feeler; then will the plate and index *V* and *X* ſhew the turns, and parts.

Fig. 2. repreſents the inſtrument immerg'd in its ciſtern of water, ready for uſe.

A B, is the ciſtern; *C*, the cover; which, when the inſtrument *fig.* 1. is rais'd upon blocks, goes on between the bar *E F* and the baſis *B C*.

D, a handle to take off the cover, when hot; *E*, the mercurial thermometer; *F*, the cock to let out the water.

G H, a hollow piece of tin, which ſupports ſeven ſpirit lamps, which are rais'd higher or lower by the ſcrews *I* and *K*, in order to give the water in the ciſtern a proper degree of heat.

A TABLE of EXPERIMENTS,

By which the numbers in Tab. I. Col. 6. N° 1, 2, *and* 4, *were determin'd.*

Experiment 1. The time elaps'd between approaching the bar to the inſtrument and taking the firſt meaſure, was half a minute: Therefore the intervals between taking the ſucceeding meaſures was half a minute alſo. The firſt meaſure was 208; the ſecond $214\frac{1}{2}$; the third $216\frac{1}{2}$; the fourth $217\frac{1}{2}$. The differences of theſe are $6\frac{1}{2}$, 2, and 1; which pretty well tallies with the three laſt terms of the following geometrical progreſſion whoſe common diviſor is 2.8; *viz.* 17.7 : 6.3 :: 2.25 : .8; therefore as the meaſures increas'd from the firſt, the firſt meaſure being diminiſh'd by the firſt term, *viz.* 208 — 17.7 = 190.3, will be the true meaſure of the bar at the firſt inſtant of its application, before it was expanded by the heat and moiſture about the inſtrument.

Exp. 2. The firſt meaſure was $221\frac{1}{4}$; the ſecond, 227; third, $230\frac{1}{2}$; fourth, $232\frac{3}{4}$; whoſe differences are $5\frac{3}{4}$, $3\frac{1}{2}$, and $2\frac{1}{4}$; agreeing with the three laſt terms of the following progreſſion, whoſe common diviſor is 1.6; *viz.* 9.2 : 5.8 :: 3.6 : 2.2; therefore 221.25 — 9.2 = 212.15.

Exp. 3. The firſt meaſure taken was 401; and at that degree of heat the wooden bar did not ſenſibly alter during two minutes.

Exp. 4. The firſt meaſure taken was $275\frac{1}{2}$; ſecond, $278\frac{1}{2}$; third, $280\frac{3}{4}$; and the fourth, $282\frac{1}{4}$: The differences are, 3, $2\frac{1}{4}$, $1\frac{1}{2}$; agreeing with the three

TAB. I. A TABLE of Experiments.

No	Matter of the Bars.				
1	Standard bar of white deal	—	—	—	
2	—	—	—	—	repeated
3	—	—	—	—	repeated
4	—	—	—	—	repeated
5	White glaſs barometer tube	—	—	—	
6	—	—	—	—	repeated
7	—	—	—	—	repeated
8	Martial regulus of antimony	—	—	—	
9	—	—	—	—	repeated
10	Bliſter'd ſteel	—	—	—	
11	—	—	—	—	repeated
12	Another bar from the ſame gad	—	—	—	
13	Another bar from a different gad	—	—	—	
14	—	—	—	—	repeated
15	Steel harden'd	—	—	—	
16	Dantzick iron	—	—	—	
17	Another bar from the ſame gad	—	—	—	
18	Another bar from a different gad	—	—	—	
19	—	—	—	—	repeated
20	Thick wire of Engliſh iron	—	—	—	
21	Another rod from the ſame piece	—	—	—	
22	Biſmuth, or tin-glaſs	—	—	—	
23	Copper plate hammer'd	—	—	—	
24	—	—	—	—	repeated
25	Another bar from the ſame piece	—	—	—	
26	—	—	—	—	repeated
27	Copper 8 parts, with tin 1	—	—	—	
28	Caſt braſs hammer'd	—	—	—	
29	—	—	—	—	repeated
30	The ſame braſs unhammer'd	—	—	—	
31	Thick braſs wire hard drawn	—	—	—	
32	—	—	—	—	repeated
33	Braſs wire ſofter drawn	—	—	—	
34	Speculum metal	—	—	—	
35	Braſs 16 parts, with tin 1	—	—	—	
36	Speltre ſolder, *viz.* braſs 2, zink 1	—	—	—	
37	Fine pewter	—	—	—	
38	Grain tin	—	—	—	
39	—	—	—	—	repeated
40	Soft ſolder, *viz.* lead 2, tin 1	—	—	—	
41	Zink, 8 parts, tin 1, lightly hammer'd	—	—	—	
42	Hard lead	—	—	—	
43	—	—	—	—	repeated
44	Soft lead	—	—	—	
45	Zink, or ſpeltre	—	—	—	
46	—	—	—	—	repeated
47	Zink hammer'd out half an inch a foot	—	—	—	

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Cold Measure.		Middle Measure.		Hot Measure.		Extremes.			Cold and Medium.			Mean Pts deduced from Extremes.	Irregularity.
Parts of Micrometer.	Deg. by Thermom.	Parts of Micrometer.	Deg. of Thermom.	Parts of Micrometer.	Deg. of Thermom.	Different Parts.	Diff. Thermom.	Diff. Pts reduced to 166°.	Different Parts.	Diff. Thermomet.	Diff. Pts reduced to 66°.		
486.3	40 ½	— —	— —	190.3	211 ½	—296.0	171	287.0	— —	— —	— —	— —	— —
495.8	47 ½	— —	— —	212.1	211	—283.7	163 ½	288.0	— —	— —	— —	— —	— —
514.6	43 ½	401.0	109	— —	— —	— — —	— —	287.0	113.6	65 ½	114.5	— —	— —
573.1	37	— —	— —	271.1	211 ½	—302.0	174 ½	288.0	— —	— —	— —	— —	— —
625.0	39 ½	569.0	96 ½	500.5	163	—124.5	123 ½	167.5	56.0	57	65.0	66.5	+1 ½
566.6	39	— —	— —	406.3	206	—160.3	167	159.5	— —	— —	— —	— —	— —
557.1	47	494.7	112	398.0	208	—159.1	161	163.5	62.4	65	63.3	65.0	+1 ½
684.1	47	— —	— —	560.0	210	—124.1	163	126.1	— —	— —	— —	— —	— —
751.0	39 ½	708.0	95 ½	662.5	154	— 88.5	114 ½	128.0	43.0	56	50.7	51.0	+ ¼
685.7	40	— —	— —	564.9	210	—120.8	170	118.2	— —	— —	— —	— —	— —
683.8	47	638.0	113 ½	566.3	210 ½	—117.5	163 ½	119.0	45.8	66 ½	45.5	47.4	+2
719.7	40	— —	— —	599.5	210	—120.2	170	117.5	— —	— —	— —	— —	— —
740.1	40 ½	— —	— —	622.3	210 ½	—117.8	170	115.5	— —	— —	— —	— —	— —
739.9	47	694.0	114	625.0	209	—114.9	162	117.5	45.9	67	45.1	46.7	+1 ½
904.5	37 ¾	855.5	113 ½	795.2	210	—109.3	172 ¼	105.3	49.0	75 ¾	42.7	41.9	—1
697.5	40 ½	— —	— —	593.5	211 ½	—104.0	171	101.0	— —	— —	— —	— —	— —
705.8	40 ½	— —	— —	604.5	210 ½	—101.3	170	99.0	— —	— —	— —	— —	— —
683.9	40 ½	— —	— —	580.3	210 ½	—103.6	170	101.0	— —	— —	— —	— —	— —
683.5	47	642.9	114	583.1	209	—100.4	162	102.5	40.6	67	40.0	40.8	+ ¾
756.0	40	— —	— —	653.6	209	—102.4	169	101.0	— —	— —	— —	— —	— —
694.9	40	— —	— —	593.6	207	—101.3	167	101.0	— —	— —	— —	— —	— —
689.6	39	— —	— —	606.0	211	— 83.6	172	80.7	— —	— —	— —	— —	— —
542.1	40	— —	— —	511.1	205	— 31.0	165	31.2	— —	— —	— —	— —	— —
605.0	39 ½	593.5	94 ¾	582.0	142	— 23.0	102 ½	37.2	11.5	55 ¼	13.7	14.8	+1
660.0	39 ½	— —	— —	627.1	203	— 32.9	163 ½	33.4	— —	— —	— —	— —	— —
717.0	39 ¾	705.0	93 ½	694.5	139 ¾	— 22.5	100	37.3	12.0	53 ¾	14.7	14.8	+ 1/10
721.0	47	715.0	111 ½	703.7	209	— 17.3	162	17.7	6.0	64 ½	6.1	7.0	+1
581.1	39 ½	— —	— —	571.8	210	— 9.3	170 ½	9.1	— —	— —	— —	— —	— —
584.2	47	582.0	112 ½	575.0	208 ½	— 9.2	161 ½	9.5	2.2	65 ½	2.2	3.8	+1 ½
682.4	39 ½	— —	— —	673.5	210	— 8.9	170 ½	8.7	— —	— —	— —	— —	— —
691.2	39 ½	— —	— —	691.0	210	— 0.2	170 ½	0.2	— —	— —	— —	— —	— —
694.5	47	695.5	112 ½	692.3	209 ½	— 2.2	162 ½	2.2	1.0	65 ½	1.0	0.9	— 1/10
741.1	39 ½	— —	— —	741.1	210	0.0	170 ½	0.0	— —	— —	— —	— —	— —
827.7	47	828.8	112	826.5	208 ½	— 1.2	161 ½	1.2	1.1	65	1.1	0.5	— ½
603 7	47	602.5	111 ½	597.6	209	— 6.1	162	6.3	1.2	64 ½	1.2	2.5	+1 ¼
615.0	47	624.2	111 ½	632.5	209	+ 17.5	162	17.9	9.2	64 ½	9.4	7.1	—2 ¼
743.3	47	764.0	109 ½	793 8	211	+ 50.5	164	51.1	20.7	62 ½	21.8	20.4	— 1 ½
742.4	47	767.1	115	818.2	209	+ 75.8	162	76.8	24.7	68	24.0	30.5	+6 ½
735.5	39	— —	— —	822.6	210	+ 87.1	171	84.5	— —	— —	— —	— —	— —
681.7	47	713.0	114	765.0	210	+ 83.3	163	84.7	31.3	67	30.8	33.7	+3
753.5	38 ¾	797.5	105	826.5	147	+ 73.0	108 ¼	111.8	44.0	66 ¼	43.9	44.4	+ ½
569.5	39	— —	— —	721.7	211	+152.2	172	147.2	— —	— —	— —	— —	— —
617.7	47	664.5	110	753.0	211	+135.3	164	136.5	46.8	63	49.0	54.3	+5 ¼
552.7	47	609.3	110	680.7	211	+128.0	164	129.5	56.6	63	59.2	51.5	—7 ¾
654.9	39	— —	— —	810.0	211 ½	+155.1	172 ½	149.0	— —	— —	— —	— —	— —
665.6	47	723.5	114 ½	812.0	210	+146.4	163	149.0	57.9	67 ½	56.5	59.4	+3
628.0	38 ¾	693.0	106 ½	744.5	150	+116.5	111 ¾	173.0	65.0	68 ¼	63.0	68.9	+ ⅛

Place this between p. 610, 611.

three laſt terms of the following progreſſion, whoſe common diviſor is 1.43; *viz.* 4.4 : 3.1 :: 2.15 : 1.5; therefore 275.5 — 4.4 = 271.1, which is the firſt meaſure corrected.

Hence, as appears by *Tab.* I. N° 1, 2, 3, and 4, Column 9. theſe experiments being properly reduc'd, agree within one diviſion of the micrometer; and the expanſion of the baſis, at a medium, is 287½ parts thereof; correſponding to 166° of Farenheit's thermometer.

TABLE II.

A Table of Expanſions of Metals,

Shewing, how much a foot in length of each, grows longer by an increaſe of heat correſponding to 180 degrees of Farenheit's thermometer, or to the difference between freezing and boiling water, expreſs'd in ſuch parts whereof the unit is equal to the 100ooth part of an inch.

1	White glaſs barometer-tube. . .	100
2	Martial regulus of antimony. . .	130
3	Bliſter'd ſteel.	138
4	Hard ſteel.	147
5	Iron.	151
6	Biſmuth.	167
7	Copper hammer'd. . . .	204
8	Copper 8 parts, mix'd with tin 1. . .	218
9	Caſt braſs.	225
10	Braſs 16 parts, with tin 1. . .	229
11	Braſs wire.	232
12	Speculum metal.	232
13	Spelter ſolder, *viz.* braſs 2 parts, zink 1.	247
14	Fine pewter.	274
15	Grain tin.	298
16	Soft ſolder, *viz.* lead 2, tin 1. . .	301
17	Zink 8 parts, with tin 1, a little hammer'd.	323
18	Lead.	344
19	Zink or ſpelter.	353
20	Zink hammer'd half an inch per foot. .	373

P. S. It is now ſeveral years ſince I firſt obſerv'd the very conſiderable expanſion of the ſemi-metallic ſubſtance call'd *zink*, *ſpelter*, or *toot-anag*; and propos'd it as more fit for the purpoſe of making compound pendulums, and metalline thermometers, than braſs; as its expanſion ſeem'd conſiderably greater, and its conſiſtence, when gently hammer'd, not much inferior. With the ſame view I have made trial of ſeveral other metallic compoſitions, beſides what is above ſet down; but they all prov'd much inferior to zink in expanſion, and moſt of them in conſiſtence.

It ſeems, that metals obſerve a quite different proportion of expanſion in a fluid, to what they do in a ſolid ſtate: For regulus of antimony ſeem'd to ſhrink in fixing, after being melted, conſiderably more than zink.

LXXX. *A Remark concerning the Sex of Holly.* *By Mr.* John Martyn, *F. R. S. Profeſſor of Botany in the Univerſity of* Cambridge.

Read May 30, 1754.

THE holly, *agrifolium*, or *aquifolium*, is deſcrib'd, by all the authors that have come to my knowlege, as bearing hermaphrodite flowers: But if an obſervation, which I have lately made, is right, this tree is male and female in different plants. I have, in my garden at Streatham

Streatham in Surrey, ſix pretty large plants, with differently-variegated leaves, now in full flower, three males, and three females, growing in pairs, and a male growing by itſelf, in another part of the garden. The female is that which has been deſcrib'd by authors, and I do not know that any one has deſcrib'd, or even taken the leaſt notice of, the male.

The male flower, as well as the female, is monopetalous, cut deeply into four ſegments, with a very ſmall empalement, divided alſo into four parts. It has four conſpicuous chives, which ſuſtain yellow ſummits, in which is great plenty of *farina*; but has nothing like either ſtyle, or ovary.

The female flower has, beſide its eſſential part, the ovary, four ſhort filaments, which have hitherto been taken for chives, or male organs of generation; but as I cannot perceive that they bear any ſummit, or yield any *farina* or fecundating duſt, I rather believe, that they are tubes, which aſſiſt in conveying the impregnating particles to the ſeeds; which opinion ſeems, in ſome meaſure, confirm'd by the germ being placed in the lower-part of the ſeed, according to Cæſalpinus, who ranges this tree among thoſe *quarum ſemina cor in inferiore parte habent.*

Ray has placed it among the *arbores flore fructui contiguo:* But, if my obſervation is juſt, it ought to be remov'd to the *arbores flore a fructu remoto.*

It muſt alſo be remov'd from the *tetrandria tetragynia* of Linnæus to the *diæcia tetrandria.* But if the four filaments in the female flower ſhould be found, on a more accurate obſervation by better eyes than I am bleſs'd with, to be real chives, and to contain a fecundating duſt; it will belong to the *polygamia.*

But

But whether the tree, which I verily believe to be purely female, is really ſo, or hermaphrodite, this I am ſure of, at leaſt, that the other is purely male; and even in this caſe my obſervation is new.

This obſervation was made on Saturday laſt, May 25, 1754. I have taken the firſt opportunity of communicating it, that, as the holly is now in flower, other botaniſts of the Society may have an immediate opportunity either of confirming or contradicting what I have here related.

Addition to Dr. Martyn's *Paper upon the Sex of the Holly.*

THE Royal Society having done me the honour of referring the conſideration of the ſex of the holly to me, in conſequence of Dr. Martyn's obſervations thereupon; I accordingly firſt examin'd, in company with that excellent botaniſt Mr. Miller, the holly-trees in the botanical garden at Chelſea. We there found, as Dr. Martyn had, that the flowers were of different ſexes; but not as thoſe in the doctor's garden, male and female upon different plants, but female and hermaphrodite upon different plants. I afterwards, both at Hampſtead, and at the duke of Argyll's at Whitton, obſerv'd ſeveral trees bearing male flowers, others female flowers. Hence it appears, that not only Dr. Martyn's obſervation of the holly being male and female in different trees is well founded, but alſo that it is male, female, and hermaphrodite, upon different trees: And I ſhould not wonder, if upon a ſtill farther examination, as in the mulberry,

that

that the male and female flowers of the holly ſhould be found, not only upon different, but upon the ſame tree: Or even, as in the *empetrum*, or berry-bearing heath, that ſome holly-trees ſhould be found bearing only male flowers, others bearing only female flowers, others only hermaphrodite flowers, others both male and female, others both male and hermaphrodite, others female and hermaphrodite, others ſtill bearing flowers male, female, and hermaphrodite, upon the ſame tree. The holly, therefore, as Dr. Martyn has juſtly obſerv'd, ſhould be remov'd, in the ſyſtem of Linneus, from the *tetrandria tetragynia*; but not to the *diœcia tetrandria*, but rather to the claſs *polygamia*, and to the order *trioicia*.

July 26, 1754. William Watſon.

LXXXI. *A Continuation of the Account of the Weather in* Madeira. *By Dr.* Thomas Heberden. *Communicated by Dr.* W. Heberden, *F. R. S.*

Read May 30, 1754.

Madeira, *Anno* 1751.

	Barometer.				Thermometer.		
	M. H.	G. H.	L. H.		M. H.	G. H.	L. H.
January	29.775	30.1	29.45		64.354	69	62
February	29.896	30.1	29.5		64.553	68	60
March	30.002	30.1	29.7		64.967	70	63
April	29.97	30.1	29.85		65.01	68	63
May	29.827	30.05	29.6		66.822	70	63
June	30	30.05	29.8	L. W.	70.833	76	68
July	29.973	30.05	29.9		74.161	77	72
Auguſt	29.942	30.05	29.9		75.709	78	72
September	29.931	30	29.8		74.916	78	73
October	29.86	30.05	29.4		72.774	76	68
November	29.813	30.1	29.6		68.533	72	65
December	29.807	30.1	29.3		63.725	71	62
	Anno 1752.						
January	29.788	30.1	29.55		65.254	68	62
February	29.866	30.15	29.7		63.071	66	60
March	30.026	30.2	29.75		66.032	68	63
April	22.844	30.05	29.45		65.683	68	64
May	29.987	30.15	29.8		66.37	69	64
June	29.966	30.1	29.7		70.58	74	67
July	29.999	30.1	29.9		72.962	75	70
Auguſt	30.002	30.05	29.9		73.86	76	73
September	30.022	30.1	29.9	L. W.	76.538	80	74
October	29.807	30.1	29.7		73.08	78	69
November	29. 51	30.05	29.75	L. W.	68.3	73	64
December	30.05	30.15	29.7		66.354	70	64

Madeira, *Anno* 1753.

	Barometer			Thermometer.		
	M. H.	G. H.	L. H.	M. H.	G. H.	L. H.
January	29.904	30.15	29.6	63.129	66	61
February	29.933	30.15	29.6	65.946	70	61
March	29.943	30.15	29.5	66.967	69	64
April	29.921	30.1	29.7	65.316	68	63
May	29.896	30	29.8	66.709	69	65
June	29.988	30.05	29.8	69.6	72	68

Collecting the reſpective ſums of the daily heights of the inſtruments throughout the year, and extracting the mean altitude, I find,

The mean altitude of the barometer for each day, is . . . . 29.915 inches.

Of the thermometer, 68°.918.

The 'greateſt barometrical variation, during 'four years and four months, has been 9 tenths of an inch, *viz.* from 29.3 to 30.2. For altho' in April 1751. the mercury ſtood at 29.1, I find, by a note in my diary, that ſome air having inſinuated itſelf into the tube, I judg'd the mercury to ſtand 3 tenths of an inch lower than it ought.

The greateſt thermometrical variation, during the ſaid time, has been 20°; *viz.* from 60° to 80°: But it may be obſerv'd, I never knew it riſe ſo high but once; occaſion'd by a very ſtrong léſte or levant wind; the extreme height, without ſuch an accident, being never more than 78°.

N. B. It may be neceſſary to adviſe, that my obſervations are made in the city of Funchal.

The

The quantity of rain, which has fallen in the iſland of Madeira.

Anno	1751.	1752.	1753.
	Inch. Dec. Pts.	Inch. Dec. Pts.	Inch. Dec. Pts.
January	5.614	11.452	5.374
February	.867	4.496	2.912
March	— —	.189	4.679
April	.011	2.479	1.604
May	1.59	.399	.038
June	.022	.860	.284
July	— —	.221	.019
Auguſt	— —	— —	— —
September	3.176	.03	1.37
October	1.707	9.897	2.352
November	3.509	5.37	1.967
December	10.483	.308	2.974
	26.979	38.701	23.573

The quantity of rain, which has fallen the laſt ſeven years, from 1747. to 1753. incluſive, amounts to . . . . . 214.346 inches.

Therefore the mean quantity for each year is . . . 30.62 +.

The mean quantity of water for each day of the ſaid ſeven years .083 +.

The number of days it rain'd in the ſaid ſeven years . . . 462

The mean quantity of water for each of the days it rain'd . .503 +.

An account of the number of days, on which it rained, and of the greateſt quantity of water, which fell in any one day of the reſpective month; in inches and decimal parts.

Anno	1747.		1748.		1749.		1750.		1751.		1752.		1753.	
	Days.	Quantity of water.	Days.	Quantity of water.	Days.	Quantity of water.	Days.	Quantity of water.	Days.	Quantity of water.	Days.	Quantity of water.	Days.	Quantity of water.
January	17	5.4	8	3.055	6	.586	15	1.855	12	1.553	14	2.751	16	.862
February	2	.306	13	2.884	3	.9	8	.474	7	.457	10	1.491	5	2.453
March	16	1.384	8	2.501	3	.573	6	.498	—	— —	2	.118	8	1.342
April	2	.328	6	.521	2	.502	1	.039	2	.006	6	.4	5	.954
May	3	.171	—	— —	11	1.018	5	.618	5	.432	2	.385	1	.038
June	3	.9	1	.42	1	.113	2	.164	1	.022	5	.523	3	.284
July	1	.2	—	— —	—	— —	1	.176	—	— —	1	.221	1	.019
Auguſt	2	.01	3	1.475	—	— —	1	.003	—	— —	—	— —	—	— —
September	4	.327	1	.81	4	.32	6	.434	3	1.5	1	.03	8	.742
October	1	.01	7	1.415	12	.387	13	.2	9	.425	16	2.438	10	.657
November	9	1.151	4	1.283	7	.912	10	.942	11	.749	13	1.031	4	.959
December	3	.925	2	1.5	14	1.5	6	.055	15	2.45	3	.257	10	.725
Total N° of days	63		53		63		74		65		73		71	

LXXXII. *Obſervations upon Father* Kircher's *Opinion concerning the burning of the Fleet of* Marcellus *by* Archimedes. *By* James Parſons, *M. D. F. R. S.*

Read June 13, 1754.

WHEN Marcellus beſieg'd the city of Syracuſe, it is well known, by the authority of thoſe great hiſtorians Livy, Polybius, and Plutarch, that the incomparable ſkill and devices of Archimedes were the only obſtacles to his ſucceeding in his enterprize, much ſooner than he did. Theſe authors tell us of his having invented machines, with which he threw ſtones of an enormous weight, into the ſhips of the beſieger; with ſhowers of darts, flints, pieces of timber, and ſuch like; and had ſo prepar'd his engines, as to be proper for any diſtance the veſſels might lie at, in the harbour. And they are minute in their deſcriptions of ſome of them; particularly, in his having deſtroy'd the *ſambuca*, a machine contriv'd by Marcellus. Nor does it appear that the forces, inveſting the city by land, far'd any better than thoſe by water; for it is ſaid he gall'd them in all quarters. And tho' the machines, as deſcrib'd by theſe great authors, were wonders, ſurpaſſing the comprehenſions of the generality of mankind, yet I believe their accounts have credit with the candid part of the learned, who delight in hiſtory and antiquities.

But what was moſt diſcredited, was Archimedes's ſetting fire to the ſhips, by a burning ſpeculum. Indeed ſo diſtinguiſh'd a genius, if he could not de-

ſtroy

ſtroy them in that manner, muſt know, that he might have thrown combuſtible matter, ſufficient to burn the galleys, from his projectile machines: For we cannot imagine that he was ignorant of every kind of theſe, and not even of the wildfire of the Greeks. But, however, to account for his burning the fleet, by a ſpeculum, was the difficult point.

When philoſophers began to increaſe their catoptrical experiments, which they did very early, they found the focus, of every ſpeculum that was concave, ſo ſhort, that they were eaſily inclin'd to conclude, that Archimedes could not ſet fire to the fleet by a ſpeculum; and hence the fact became intirely diſcredited, till the famous Kircher, and his pupil Schottus, whoſe characters and works the learned world are well enough acquainted with, reſolv'd to conſider not only the ſtory of Archimedes, but alſo that of Proclus, who is ſaid to have deſtroy'd a fleet at Conſtantinople in the ſame manner.

Kircher, however, notwithſtanding the incredulity that appear'd every-where among the learned of his time, concerning thoſe facts, was not deterr'd from giving great attention to the matter himſelf; which led him to make innumerable experiments, in order to ſee whether it was poſſible to be done or not, before he would give any opinion about it; and at length, when he had commended the parabolical ſpeculum, which he, and others, were inclin'd to think the moſt likely to ſucceed in ſuch an enterprize; he was inclin'd to think, Archimedes made uſe of ſuch a ſpeculum.

But, ſoon after, he was diſcontented with this notion, and began to make new eſſays; and, being happy

happy in his invention, he fell upon one, which leſſen'd his former good opinion of the parabolical ſpeculum, and made him more ſenſible of the inconveniencies attending it, or thoſe of any other form, that had any great degree of concavity; and, in a word, engag'd him intirely in favour of his new thought, which was put in execution in the following manner:

He erected a frame, on which he placed five plane ſpecula, of equal given dimenſions, with ſuch inclinations as made them all throw their reflected rays upon the ſame place, at more than one hundred feet diſtance. When he had ſet the firſt ſpeculum, he went and laid his hand upon the place, whereupon he caus'd the rays to fall, and found it warm; when he added thoſe of the ſecond, the heat was doubled; the third increas'd the heat in the ſame proportion; and the fourth being added, the heat was ſcarce to be borne; but the fifth made it intolerable. From whence he concludes, that, by multiplying thoſe ſpecula, the heat might be ſo increas'd, as to ſet fire to combuſtible matter at greater diſtances, according to the number apply'd.

Now becauſe I think it a matter of ſome conſequence, in the learned world, to aſcertain to every author the praiſes due to his labours and diſcoveries, and to ſhew this author's application of the invention to the confirmation of this Archimedean fact; I think it alſo incumbent on me to give the Society his own words upon it, which he himſelf has reduced to a problem.

PROBLEMA

Problema IV.

Machinam ex ſpeculis planis conſtruere ad centum pedes & ultra urentem.

" Suppono igitur primo, ſpeculum planum tanto
" majorem lucem reflectere, quanto illud majus fu-
" erit; ita pedale ſpeculum in vicino pariete, lucem
" pedalem, in remoto, ad centum pedes lucem tan-
" tam, quanta pars quarta pedis eſt, projiciere, expe-
" rientia comperi. Supponendum ſecundo infinitos
" radios, ex ſingulis ſpeculi punctis, reflexos, hanc
" lucem conſtituere. Si itaque aliud ſpeculum pla-
" num ita conſtituas ut reflexa lux duplicatæ paulo
" ante luci congruat, dico & lucem & calorem tri-
" platum iri, & ſic in infinitum procedendo. Sup-
" ponendum tertio, lucem & calorem hujuſmodi ſpe-
" culorum reflectione, in unum ſpatium reflexum,
" pro multitudine ſpeculorum multiplicari; quem-
" admodum fuſe oſtendimus lib. 2° de Actinoboliſmis,
" part. 1ª. Ego certe hujus rei in quinque ſpeculis
" experimentum ſumpſi; & prima quidem lux, a
" luce directa, diverſum calorem habebat; duplata
" lux notabile caloris augmentum jam ſuſcipiebat;
" triplata calorem ignis præferebat; quadruplicata
" calorem utcunque tollerabilem adhuc præſtabat;
" quintuplicata, pene intolerabilem; unde certo &
" indubitate concluſi, multiplicatis ſpeculis planis, &
" ea ratione collocatis, ut omnia, reflexam ſolis lucem,

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* Kircheri Ars magna lucis & umbræ, lib. X. pars III. cap. i. diſtinctio 3.

" in

" in unum ſpatium cogant, futurum, ut non tantum
" majorem uſtionis effectum, quam quælibet uſtoria
" parabolica, hyperbolica, elliptica, præſtent; ſed &
" in multo majus ſpatium, radioſam lucem reflectant,
" quemadmodum me in quinque ſpeculis ad ſpatium
" centum & amplius pedum, experientia docuit."

Schottus gives the ſame account of Kircher's experiment. He accompanied him in all his trials, as well as in his journey to Syracuſe, after he had brought his plane mirrours to anſwer his purpoſe; and, upon viewing the place, they both concluded, the galleys of Marcellus could not be farther than thirty paces from Archimedes. And yet Schottus declared, that if a concave ſpeculum could be conſtructed, as large as the rotunda, it could not have a ſufficient focus to effect what both Archimedes and Proclus are ſaid to have done.

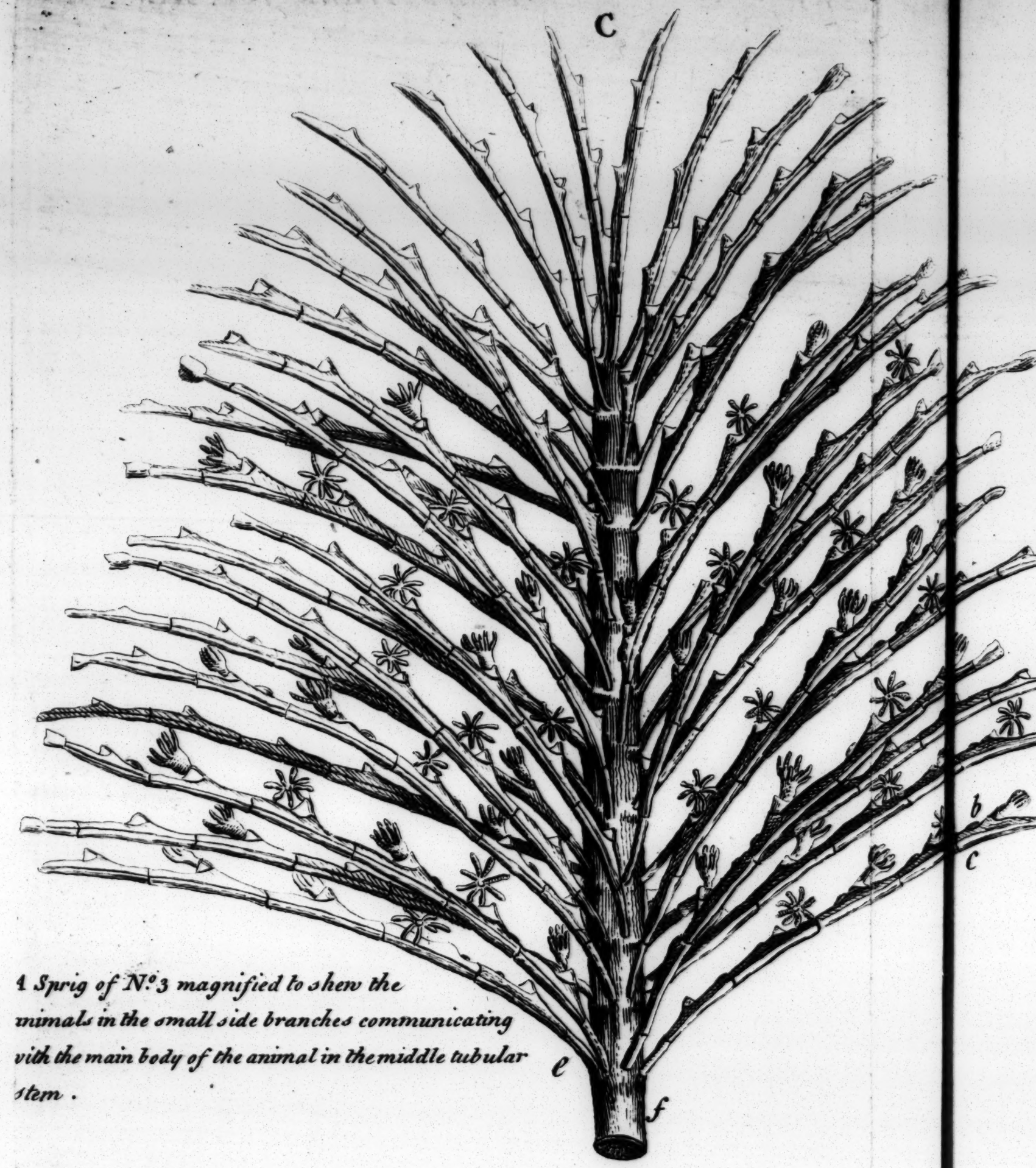
Thus we ſee Kircher had ſcientifically eſtabliſh'd the problem, for the conſtruction of a burning machine, conſiſting of any number of plane ſpecula; which was afterwards farther confirm'd by the ingenious Monſieur de Buffon, a worthy member of our Society, at Paris; as it appears in two letters, one from Mr. Needham, fellow of the Royal Society, to me; and the other from the marquis Nicolini to our late worthy preſident; both read before this Society in April 1747, and ſince printed in theſe *Tranſactions*; in which Monſieur de Buffon is ſaid to be the inventor *. If ſo, we cannot ſuppoſe he could have ſeen what either Kircher or Schottus had wrote about it.

* Since this paper was committed to the preſs, the author has found, that Monſ. de Buffon, at the cloſe of his diſcourſe on this ſubject, printed in the *Memoirs of the Royal Academy of Sciences*, has mentioned that himſelf had ſolved the problem, before he knew that it had been done by Kircher.

LXXXIII. *An Account of ſeveral Bones of an Elephant found at* Leyſdown *in the Iſland of* Sheppey. *In two Letters from Mr.* Jacob, *Surgeon at* Feverſham, *to* James Parſons, *M. D. F. R. S. dated* May 24, 1754.

SIR,

Read June 13, 1754. THREE or four years ago I ſent you the acetabulum of an elephant, which was diſcover'd ſticking in the clay, which was partly waſh'd away from the cliff, at a place call'd Leyſdown, in the iſle of Sheppey, a full mile eaſtward of the cliffs of Minſter. This, with other parts, as one of the ſpinal vertebræ, a thigh-bone four feet long, and numberleſs other fragments too rotten to be then taken up intire, I ſaw; all which lay below high-water mark: And as the place, and ſome adjacent land, ſoon after, became my property by a purchaſe I made; I went, attended by workmen, in ſearch of more, and found an elephant's tuſk; and, as it lay intire to appearance, took its dimenſions; which were, in length, eight feet; and in circumference, in the middle, twelve inches: But it fell all to pieces, when we endeavoured to raiſe it. I found, alſo, part of a ſcapula, its ſinus almoſt intire, and three inches diameter. I have ſince found ſome pieces of grinders, and, by care, hope for more; and have now a larger one, from another part of this iſland. The pyrites, however, abounds ſo much in the clay wherein this

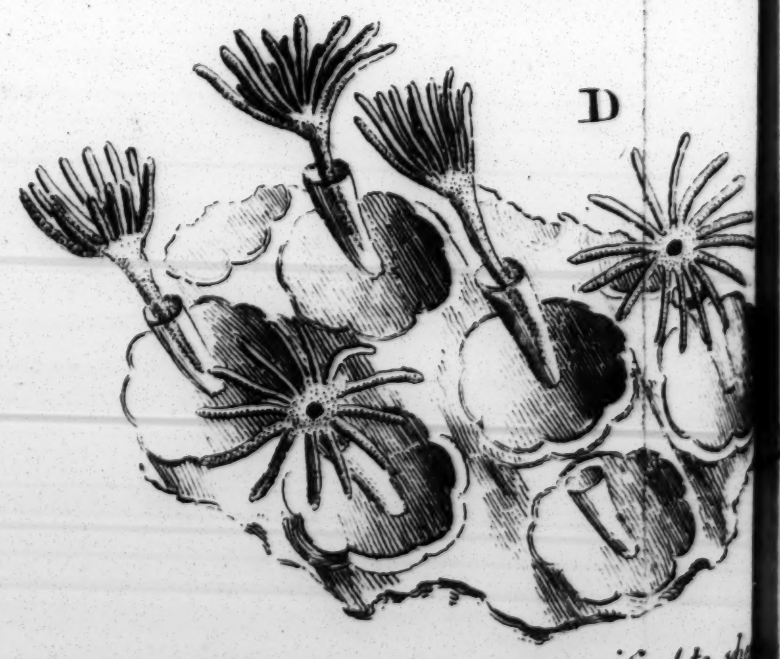


1 Sprig of N.o 3 magnified to shew the animals in the small side branches communicating with the main body of the animal in the middle tubular stem .



Fig. N.o 4.

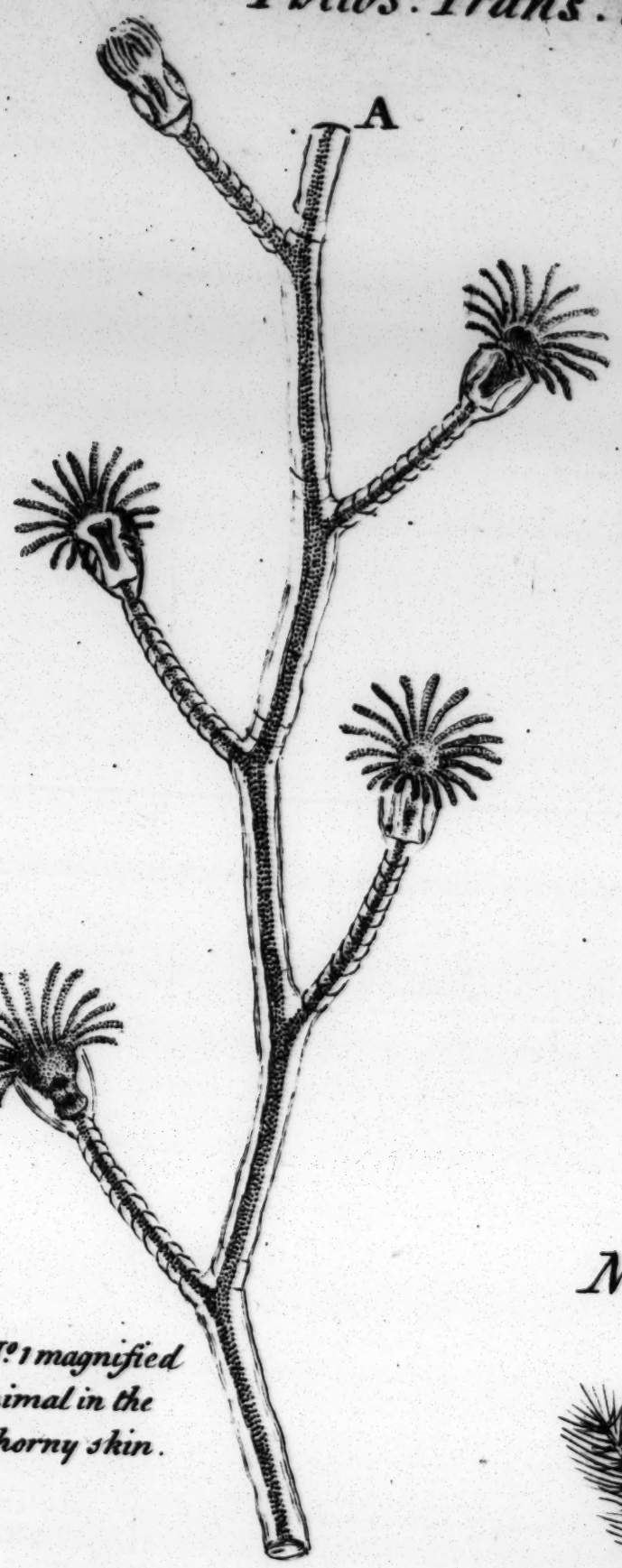
Cells of the common Sea Insect on a Fucus .

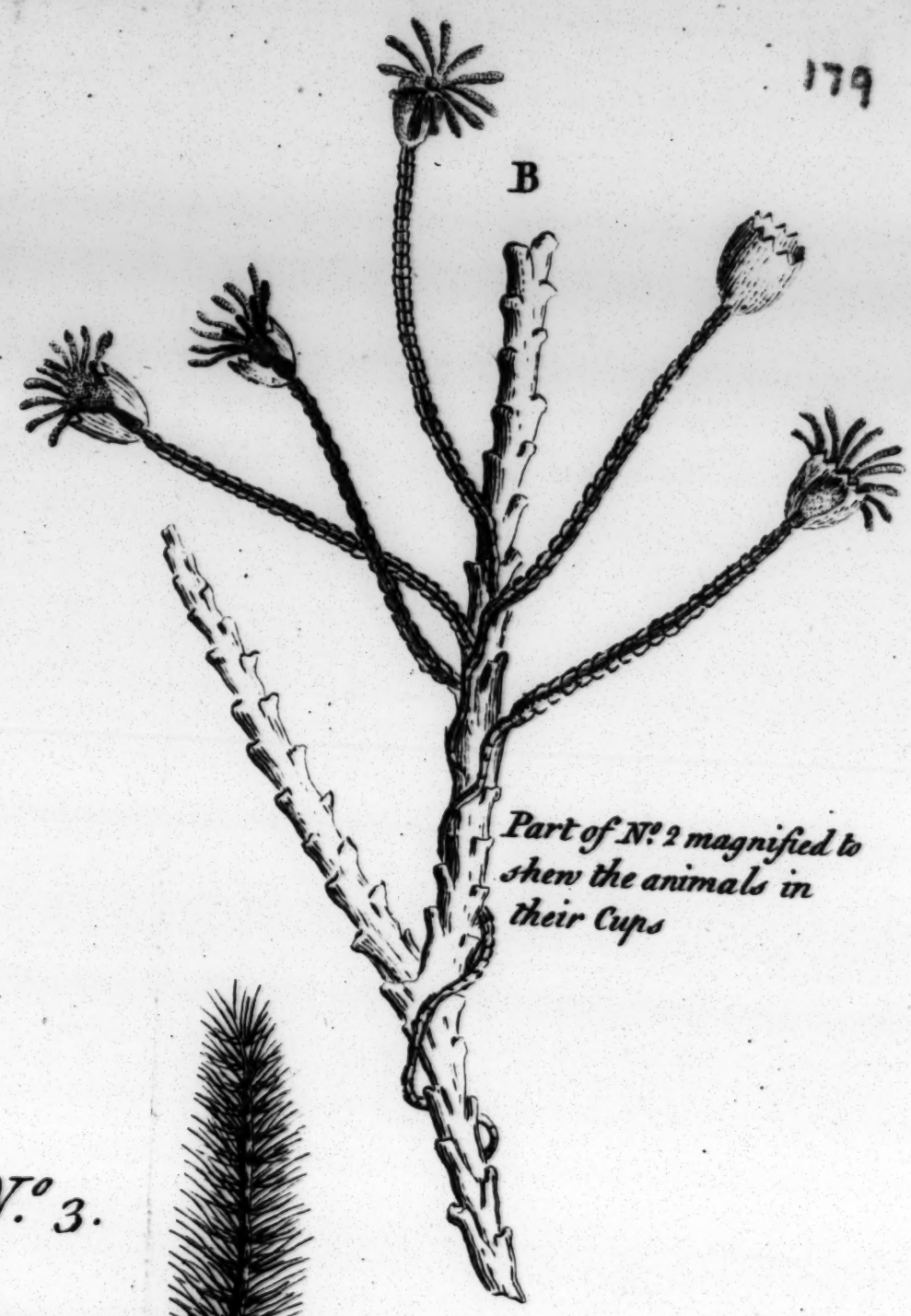


A few of the cells at N.4 magnified to sh the figure of the animals in them



A Sprig of N.o 1 magnified to shew the animal in the inside of it's horny skin.

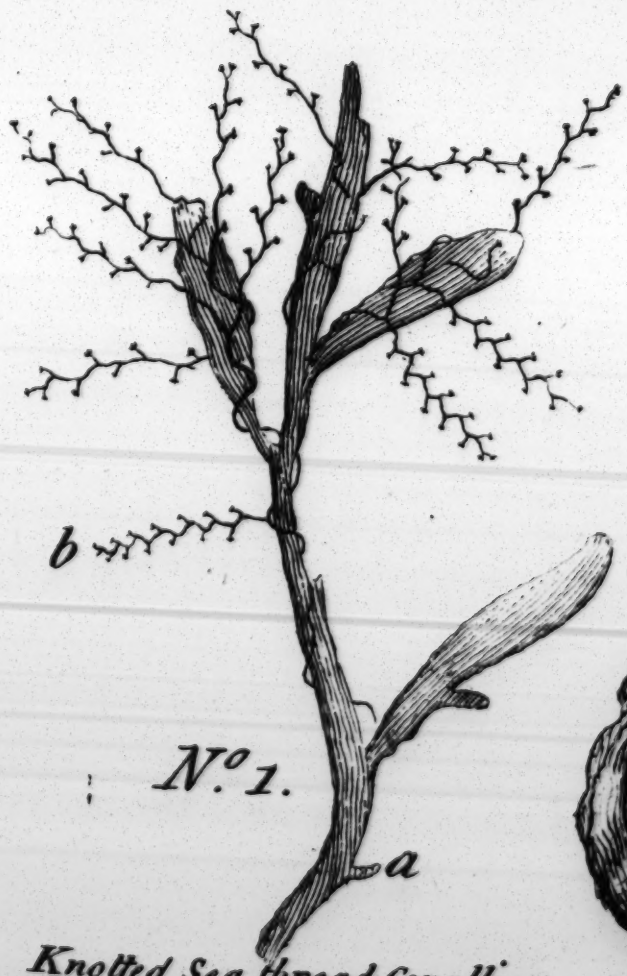




Part of N.o 2 magnified to shew the animals in their Cups

N.o 3.





N.o 1.

Knotted Sea thread Coralline climbing up a Fucus.



Fig. N.o 2.

Climbing Coralline with bell shap'd Cups

Branch'd Lobsters horn Coralline on an Oyster shell. J. Mynde sc.

this animal was embedded, that I deſpair of finding any whole bones: But I think theſe fragments are ſufficient to ſhew, that the elephant was as large as that mention'd by Fentzelius, in theſe *Tranſactions*.

The apex of the tuſk, which I preſerv'd, and the acetabulum before you, were both found within twenty feet of the other bones mention'd, and are, as I apprehend, in better condition than they are, from their being taken up immediately upon being diſcover'd, and not left to be expos'd to the injury of the weather, and violence of the tides; which ſoon affects bodies ſo expos'd, after having lain under ground for ages.

LXXXIV. *A Letter from Mr.* John Ellis, *F. R. S. to Mr.* Peter Collinſon, *F. R. S. concerning the animal Life of thoſe Coral-lines, that look like minute Trees, and grow upon Oyſters and Fucus's all round the Sea-coaſt of this Kingdom.*

SIR,

Read June 13, 1754. THE doubts, that I find ſtill remain on the minds of many curious and learned men of the animal nature of corallines, on account of their beautiful ramifications, and regular plant-like appearances, determin'd me to perſuade our ingenious friend, Mr. Ehret, to accompany me to the ſea-ſide, that he might there be an eye-witneſs of what I had advanced to you, and many other

worthy members of the Society; and to make exact drawings of the ſeveral different objects, as they appear'd to him thro' the microſcope.

Accordingly, on Monday the third of this inſtant June, we ſet out, and arriv'd at Lewes in Suſſex that evening, and the next morning at Brighthelmſtone. The weather being very calm, and few fucus's or corallines being thrown aſhore on the beach, I hir'd a fiſherman, the next day, to take up ſome oyſters from an old oyſter-ground, that had been long diſus'd, lying about three or four leagues off to ſea, and where, by his deſcription, the ſhells were cover'd with great varieties of theſe minute tree-like corallines; with directions, that, as ſoon as he took them out of the ſea, he ſhould immediately put them into a bucket of ſea-water; but, unfortunately, he put the oyſters into a fiſherman's baſket; by which means, many varieties were dead, notwithſtanding we receiv'd them two hours after they were taken out of the ſea, and had them put immediately into ſea-water: However, by the oyſters lying on one another, ſome of the corallines were kept ſo moiſt, as to be perfectly alive, and briſk. In order to diſtinguiſh them more eaſily, we pluck'd them off the oyſters, and placed them in white earthen plates, and pour'd as much ſea-water over them as would juſt cover them: After we had let them reſt for a little while, to recover themſelves, we could eaſily diſcover, with a magnifying glaſs of an inch focus, which were alive, and which not: Accordingly, I cut off ſmall pieces of ſeveral of the livelieſt, and placed them in watch-glaſſes fill'd with ſea-water; theſe, after reſting a little while, I placed, one after another, on the ſtage of the microſcope.

The

The unuſual ſight ſo amaz'd our friend (who had his doubts), that he could ſcarce believe his own eyes; for he had hitherto imagin'd, with many others, that theſe corallines were vegetables, and only the receptacles of animals, as many other plants are, and not the proper caſes, ſkins, or coverings, of their bodies.

The firſt coralline that offer'd itſelf to our view, was *n°* 1. (*Plate* XXII.), where it is repreſented, in its natural appearance, climbing upon the podded fucus *a*, with irregular threadlike ramifications, as at *b*; one of which is exhibited magnified at *A*, in which we may obſerve a broad dark line in the middle of the tranſparent ſtem and branches. This is part of the tender body of the animal, and ſeems as a ſupport for its ſeveral heads and ſtomachs, with the many hands or claws belonging to each: For at the top of each of the branches we may obſerve a polype with twenty tentaculi, or claws, which do the office of hands, its mouth in the centre of them, and its ſtomach underneath, inclos'd in a fine tranſparent cup. The fine out-lines repreſent the horny ſkin, or outward coat, that ſerves this compound animal as a defence, in the ſame manner as the ſhells of teſtaceous or cruſtaceous ſea-fiſh.

The ſkin or covering of the arms, that ſupport the cups, is form'd in ſmall rings, which gives the animals the more freedom to move about dextrouſly in ſeizing their prey.

At letter *B* is the microſcopical repreſentation of a ſtill ſmaller coralline than the former; the natural ſize of it is expreſs'd at *fig.* 2. This creeps up, and twines round other corallines by ſmall vermicular tubes, and ſends out its curious ſlender arms irregularly:

larly: Theſe arms, in the microſcope, look like rows of the ſmalleſt beads of a necklace: To the top of each of theſe is fix'd a cup, for the reception of the polypes, the brim of which is curiouſly indented. Theſe we ſaw alive, and extending themſelves about in various directions.

Fig. n° 3. repreſents part of another coralline of its natural ſize, juſt as it appear'd expanded in a plate of ſea-water.

It is call'd, in Ray's *Synopſis*, ed. 3. *corallina ramoſa cirris obſita*; and by Doody, in Ray's *Synopſis*, ed. 2. *fruticulus elegans geniculatus cirris obſitus.* Letter *C* expreſſes a branch of this coralline magnified; where you may obſerve, on each capillary ſide-branch, rows of ſmall polypes, each with eight tentaculi, or claws, riſing out of little ſockets. The upper diviſion, or tube, of theſe little branches, as at *b*, appears full of joints, one to each polype; but we could eaſily perceive, that all the polypes were connected together, and communicate with the principal ſtem, or body, which is inclos'd in the middle tube. The under ſmall tube of the capillary ſide-branch at *c*, which runs parallel with the upper one *b*, and adheres to it, appear'd clear, hollow, and jointed.

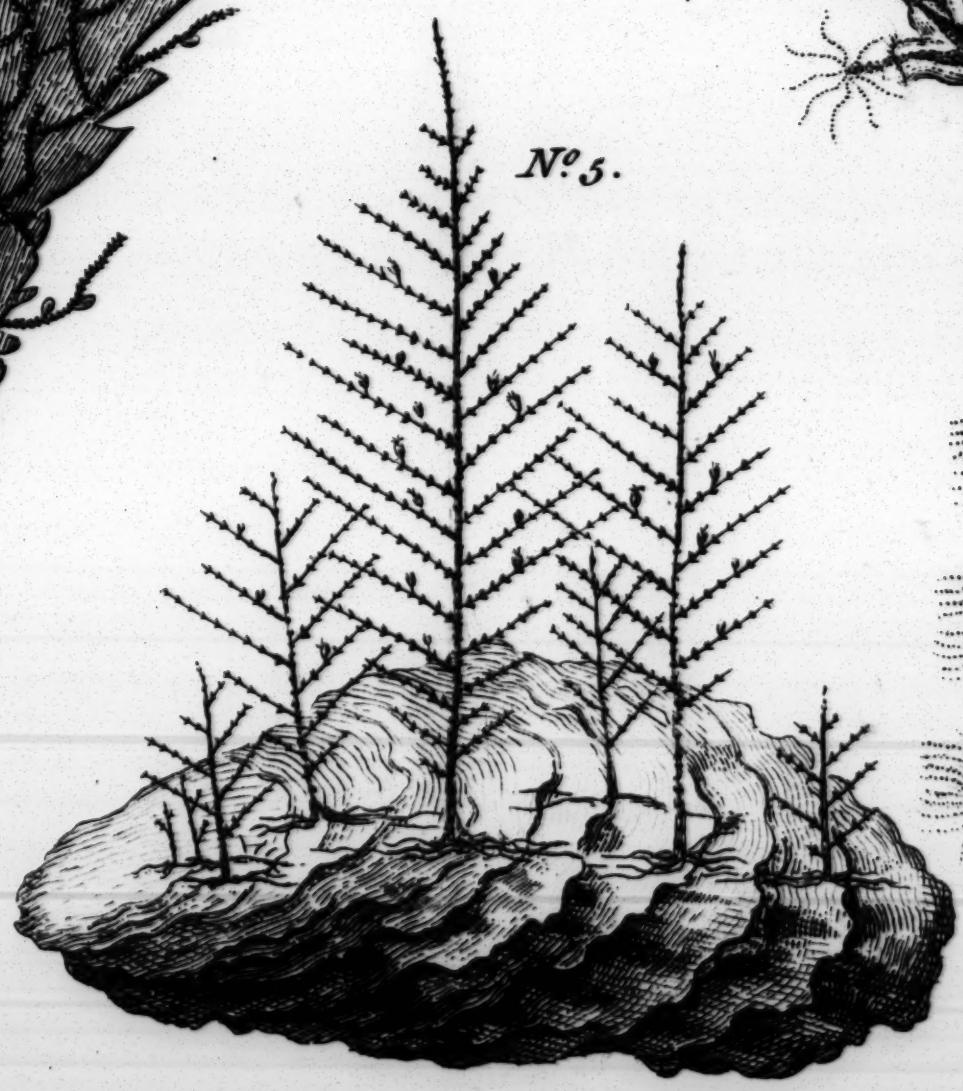
This coralline ariſes from a tuft of ſmall irregularly-matted tubes, like a ſponge growing to an oyſter-ſhell, as at *g*; the ſmaller branches *e* are inſerted in circles round the larger branch *f*, at equal diſtances, like the plant call'd horſetail, or *equiſetum.* As we were obſerving theſe corallines, we perceiv'd, on one of them, a different-ſhap'd polype, which thruſted itſelf out of a ſmall funnel-ſhap'd pipe: This was in-

ſerted



Fig. N°6.

Sea Oak Coralline. creeping on a peice of a Fucus so call'd.



N°5.

Pomgranate flowering Coralline On an Oyster shell.



Sea Oak Coralline to shew the animal and vesicles.

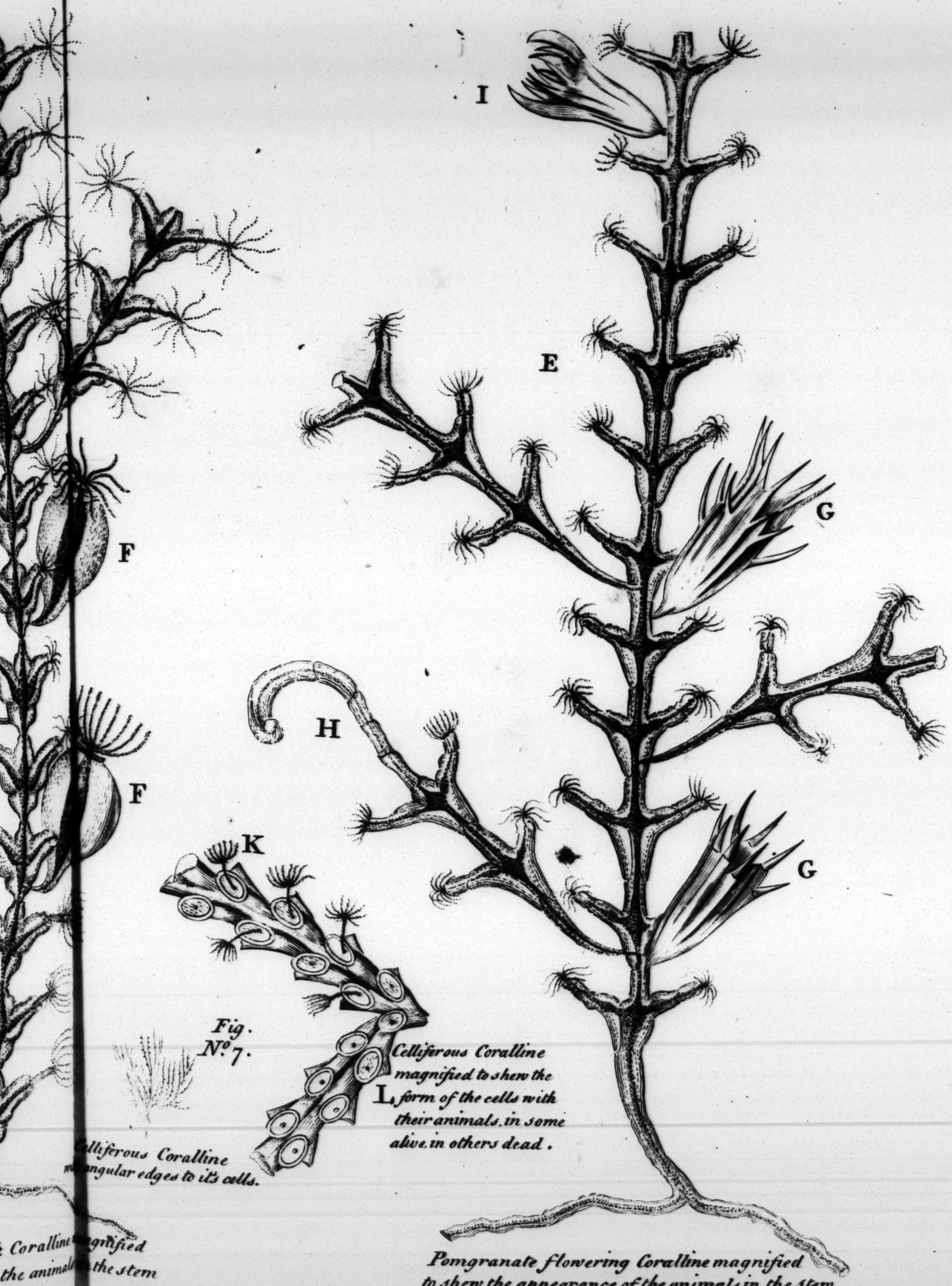


Fig. N°. 7.

Celliferous Coralline magnified to shew the form of the cells with their animals, in some alive, in others dead.

Celliferous Coralline with angular edges to it's cells.

Coralline magnified the animals in the stem cles.

Pomgranate flowering Coralline magnified to shew the appearance of the animals in the stem and the different form of the vesicles.

J. Mynde sc.

ſerted in a cell, whoſe brim, or border, was ſurrounded by little ſpines.

Theſe cells we obſerv'd to compoſe that ſpongy rough matter, which incruſts almoſt all marine ſubſtances, but chiefly fucus's. *Fig.* 4. repreſents the natural ſize of theſe cells on a fucus; letter *D* expreſſes the cells and polypes, with twelve tentaculi to each, as they appear magnified; where the animals are ſeen raiſing and expanding themſelves. When they are diſturb'd, they draw themſelves within their ſheath, or pipe, which cloſes on them, and ſink together into their cells.

The curious denticulated coralline at *n°* 5. *Plate* XXIII. has very much the appearance of a plant, at firſt view, even when it is magnified, as at *E*. This gave us a farther corroborating proof, that theſe extraordinary ſpecies of beings are animals: For we obſerv'd, that the ſmaller polypes, that extend themſelves out at the opening of every oppoſite denticle, or little projecting tube, are united at the bottom, or lower-part, to the fleſhy ſubſtance of the main body, that paſſes thro' the middle of each branch, or ſtem, and are ſo many different bodies united in one; acting like ſo many ſets of hands, placed in form of a circle, collecting food, each for a mouth in the centre, to convey nouriſhment to ſo many ſtomachs, which are fix'd in the ſwelling part, or bottom, of each denticle. This great ſupply of nouriſhment from all ſides, gives that great increaſe, and variety of ramifications, to this wonderful claſs of many-bodied animals.

Beſides theſe ſmall polypes, which compoſe the branches, theſe corallines ſend forth, from ſeveral

parts, many veſicles, of different ſhapes, at certain ſeaſons of the year, according to their different ſpecies.

Theſe veſicles are protruded from the outward ſkin or horny covering of theſe branched polypes, and from the inward or fleſhy part ariſes a larger polype; one of which occupies each of theſe veſicles.

Thus a coralline full of veſicles looks like a plant full of bloſſoms, which, after they have arriv'd at their perfect ſtate, fall off, with their capſules or veſicles, and become new-detach'd animals, to provide for themſelves; in the ſame manner as the falling ſeeds produce other plants.

Upon examining this coralline, we found, that the animals in the veſicles were dead; but, immediately afterwards, we had an opportunity of diſcovering the veſiculary polypes alive, in another coralline; which are deſcrib'd at *fig.* 6. in their natural ſize, and at letter *F* as they appear'd magnified. This ſpecies I have call'd the ſea-oak coralline, from its being moſt frequently found creeping on, and adhering to, the largeſt ſpecies of the *quercus marinus*, or ſea-oak fucus.

The veſicles of the denticulated coralline, letter *E*, are deſcrib'd as they appear'd full of ſpines at the top, and clos'd up, as at letter *G*.

The veſicles of the ſame ſpecies are more frequently found as deſcrib'd at *I*, where the ſpines are not unfolded: From this appearance, I have call'd it the pomegranate-flowering coralline, becauſe they nearly reſemble the opening bloſſom of the balauſtine, or double flower of the pomegranate.

The branches of this coralline are often obſerv'd to end in vermicular tubuli, as at *H*, which are much

of

of the ſame form with thoſe it begins with; ſo that theſe animals can, and do, change their ſhapes, for the ſeveral ends and purpoſes of their being; and this in a moſt ſurpriſing manner.

I had farther an opportunity of examining ſome of thoſe kind of corallines, which I call celleferous, from their having rows of cells diſpos'd in plant-like ramifications. The ſmall black ſpots in each cell, which I had conjectur'd before to be the embrio of a future teſtaceous animal. Vid. *Phil. Tranſ.* Vol. 48. *Tab.* VI. p. 115. I found now to be the contracted bodies of dead polypes; for we here ſaw ſome of theſe polypes alive, and extending themſelves out of their cells, as at *K*, *fig.* 7. and upon reviewing them, when they were dead, found they made the appearance of blackiſh ſpots in each cell, as at *L*, *fig.* 7. So that we have reaſon to ſuppoſe, that this ſpecies of polypes, that form theſe corallines, do change into teſtaceous bodies. But of this I do not doubt, that time, and more obſervations, will fully convince the curious. I am,

SIR,

Your moſt affectionate friend, and ſervant,

Lawrence-Lane,
June 13, 1754.

John Ellis.

LXXXV. *Extract of a Letter from* Camillo Paderni, *Keeper of the* Muſeum Herculaneum, *to* Tho. Holles, *Eſq*; *dated at* Naples, April 27, 1754.

Read June 13, 1754.

ONE reaſon for not anſwering your letter earlier has been the embaraſſments, which I have had about the muſeum, eſpecially in forming the place itſelf to a convenient ſhape for the things, which it is to contain. Till this was done, the whole was unavoidably in great confuſion. I had the direction likewiſe of the people, who work'd under-ground, and of the ſeveral artiſts otherwiſe employ'd. Beſides, I was deſirous to ſend you the printed catalogue of the muſeum, which has long been order'd, yet cannot ſtill be publiſh'd theſe three or four months. When it is out, I ſhall be at liberty to write more fully and freely than is at preſent permitted. However, I can now ſend you an account of the diſcoveries made of late, that is, within theſe twelve months, in the pretended city of Herculaneum. I ſay *pretended*, becauſe it is my own opinion, that the place, where they have dug for ſome time paſt, and actually do now dig, is not Herculaneum, but a different place from it, tho' almoſt contiguous to it; as I could eaſily make appear, was I at liberty to write: But time will clear up this matter. My duty made it neceſſary for me to deſcend into it almoſt daily; and when my buſineſs was done, I always indulged my curioſity and genius in viewing and examining the ſeveral objects there. The place

where

where they are digging, at preſent, is under *Il Boſco di Sant' Agoſtino*, but a little diſtant from the royal palace at Portici. Its depth is 125 Neapolitan palms*, one of which is more than the mercantile canna of Rome. All the buildings diſcover'd in this ſite are noble: Many of the pavements are of moſaic, variouſly and finely made; others are of different-colour'd marbles, diſpos'd with a beautiful ſymmetry; and moſt of them are already taken up. In one of theſe buildings there has been found an entire library, compos'd of volumes of the Egyptian Papyrus, of which there have been taken out about 250; and the place is not yet clear'd or emptied, it having been deem'd neceſſary to erect props firſt, to keep the earth, which lies above it, from falling in upon it. Theſe volumes of Papyrus conſiſt of Latin and Greek manuſcripts; but from their brittleneſs, occaſion'd by the fire and time, it is not poſſible to unrol them, they being now decay'd, and rotten. His majeſty however has done his part; having ſent for a certain monk from Rome, who belong'd to the Vatican library; in hopes, by his means, to have unfolded them; but hitherto in vain.

Your ſervant Paderni alone can ſhew ſome fragments of ſeveral lines; and more than this, he is much afraid, will never be ſeen. Of theſe there are many in my cuſtody, which I ſuppoſe you will have the pleaſure of obſerving, in the intended catalogue. There have been found of thoſe ſmall tables, which they cover'd with wax and the *palimpſeſton*, and then

* A Neapolitan palm is ſaid to be 11⅜ inches Engliſh.

wrote on them with the *ſtylus:* but all theſe are become a kind of cinder; and have likewiſe ſuffer'd by the damps; from both which circumſtances they are now ſo tender, that they break with the touch.

In the ſame place there have been met with three ſmall buſts; one of Epicurus, another of Zeno, and the third of Humachus; with the names of each inſcrib'd upon the baſis, in Greek letters. A little diſtant from the preceding ſite has been diſcover'd another noble building, with a ſquare court belonging to it; the inſide of which, alone, has been hitherto examin'd. This ſquare is form'd with fluted columns made of brick that was ſtucco'd. In the angles were four terms of marble, with buſts upon them, in bronze, of the fineſt manner, having the name of the Greek workman upon one of them. In the centre, betwixt the terms, there was a ſmall fountain, form'd by a vaſe ſhap'd like a cockle-ſhell, and ſupported by a ſmall fluted column. There have been likewiſe found three other buſts, large, and in bronze alſo, of the moſt excellent workmanſhip. Within theſe few days the following things have been taken out of the ſame ſite; to wit, a female ſtatue, ſix palms high, perhaps a goddeſs, tho' without any attribute, and but of middling workmanſhip; two moſt beautiful candleſticks, ſix palms and an half high, exquiſitely wrought in chas'd work; other candleſticks, much damag'd by the fire and time; many fragments in bronze, which, not having any particular merit, it is needleſs to deſcribe, except two ſmall figures of fawns, that are finely executed. In the ſame place was diſcover'd a large fountain, lin'd throughout with lead; round it were eleven heads of lioneſſes, out of which the water

flow'd.

flow'd. Pipes of lead are very often met with; and a day ſcarce paſſes, but ſomething is brought to be depoſited in my cuſtody. The above is all I can write, at preſent, concerning our late diſcoveries. The muſeum goes on increaſing greatly. Now there are in it two curule chairs, which are uniques; theſe were in a chamber by themſelves, and had been ſomewhat injur'd. One is ſimply of bronze, and the other of bronze, which had been gilt. I can aſſure you, that when, in the courſe of a few months, every thing is properly ranged and placed in this muſeum, it will be one of the fineſt in Europe; as well for the ſingularity, as beauty, of its objects. Now, particularly, I wiſh you were here; being thoroughly perſuaded, that you would paſs entire days with me in it, as ſeveral other of my friends have done, with the utmoſt ſatisfaction. If my time had not been ſo entirely taken up in putting the muſeum into ſome general order, I could eaſily have given your learned friend, Dr. Ward, the deſir'd information about the antient weights and meaſures; which particular things, however, are not yet diſtinctly methodiz'd. We have various meaſures, but without any marks upon them: By their form, they appear ſuch, as are deſcrib'd by Montfaucon, and others. We have, alſo, many weights of all ſizes, with their ſeveral marks upon them, proper for ſcales; and many ſteelyards, likewiſe, of all ſizes, the links of which being broken, it will firſt be neceſſary to reſtore them, before any proofs can be made of them. I already conceive a high eſteem for Mr. Stuart and Mr. Rivet, from the honour, which they have acquir'd to themſelves by their celebrated undertaking. When they come hi-

ther,

ther, I ſhall endeavour to procure them ſubſcribers to their work, and ſhew them every civility in my power.

LXXXVI. *Experimental Examination of a white metallic Subſtance ſaid to be found in the Gold Mines of the* Spaniſh Weſt-Indies, *and there known by the Appellations of* Platina, Platina di Pinto, Juan Blanca. *By* William Lewis, *M. B. F. R. S.*

PAPER I.

Read May 30, 1754.

Experiment 1.

THE ſubſtance brought into England under the name of *platina* appears a mixture of diſſimilar particles.

The moſt conſpicuous, and by far the largeſt part of the mixt, are, white, ſhining grains, of ſeemingly ſmooth ſurfaces, irregular figures, generally planes with the edges rounded off. Upon examining theſe with a microſcope, the ſurface appear'd in ſome parts irregular; the prominencies ſmooth, bright, and ſhining; the cavities dark-colour'd and roughiſh. A few of them were attracted, tho' weakly, by a magnetic bar.

The grains above deſcrib'd are the true *platina*. The heterogeneous matters intermingled among them, in the ſeveral parcels, were,

1. A

1. A blackiſh duſt, ſeparable by a fine ſieve. This was farther divided, by a magnetic bar, into two different ſubſtances: the part attracted, was of a fine ſparkling black colour, much reſembling the black ſand from Virginia: the part not attracted was of a dark browniſh hue, with ſeveral bright *moleculæ*, which appear'd to be fragments of the grains of platina.
2. Among the larger grains, ſeparated by a coarſe ſieve, were obſerv'd ſundry irregular dark-colour'd particles, ſome blackiſh, others with a caſt of browniſh red, in appearance reſembling fragments of emery or loadſtone. Several were attracted, weakly, by the magnet.
3. There were a few rough yellow particles, reſembling gold, which upon farther examination they were found to be, tho' probably not entirely free from platina.
4. A few globules of quickſilver, containing gold, with ſome particles of platina intermix'd and pretty ſtrongly adhering.
5. Some fine tranſparent particles, probably ſpar.
6. A very few irregular particles, of a jet-black colour. Theſe broke eaſily, and look'd like the finer kinds of pitcoal. Laid on a red-hot iron, they emitted a yellowiſh ſmoke, and ſmelt like burning coal.

Remarks. 1. It appears from the foregoing obſervations, that this mineral has not come to us in its native form; being, probably, taken out of the mines in large maſſes, which have been broke, and treated with

with mercury, in order to extract the gold, of which poſſibly it at firſt contain'd a conſiderable quantity. The quantity left by the workmen is extremely ſmall; ſome pounds of the mixt having yielded only a few grains. A moderate fire renders more of theſe golden particles diſcoverable, than can be ſeen at firſt; the mercury evaporating, by which ſeveral of them were concealed.

2. Some part of the browniſh powder is probably adventitious, as well as the mercury; being worn off from the ſtampers and mills employ'd for comminuting the mineral, and triturating it with the mercury.

3. The roughneſs and dark colour of the cavities of the grains of platina ſeem to proceed from a ſubſtance ſimilar to the black duſt, adhering in them. It is probably owing likewiſe to this heterogeneous magnetic matter, that ſome of them are attracted by the loadſtone.

Experiment 2.

Some of the purer grains of platina, by gentle ſtrokes of a flat hammer, upon a ſmooth anvil, bore to be conſiderably flatten'd, without breaking or cracking about the edges: ſome quickly crack'd, and diſcover'd internally a cloſe granulated texture. All are reducible, by rude ſtrokes in an iron mortar, tho' with difficulty, into powder. They ſeem'd to be rather more brittle when ignited, than when cold.

Experiment 3.

The ſpecific gravity of platina, with its heterogeneous admixtures, as brought to us, was found to be

be to that of water as 16.995 to 1.000. The quantity weigh'd for this purpoſe was no leſs than 2000 Troy grains.

The larger grains of platina, ſeparated as much as poſſible from the other matters by the ſieve, and cleans'd by heating, boiling in aqua fortis, mixing them with ſal ammoniac, and forcing off the ſalt by fire, and afterwards waſhing them; weigh'd in air 642, in water 606.75: whence their gravity turns out 18.213. The microſcope ſtill diſcover'd a conſiderable portion of blackiſh matter in their cavities.

Theſe trials were ſeveral times repeated on different parcels of platina: the reſult was nearly the ſame in all.

Remark. The gravity of this mineral, great as it appears to be from the foregoing experiments, would probably turn out ſtill greater upon a farther purification of the platina, ſince it is manifeſtly mix'd with ſome of the lighter heterogeneous matters.

Experiment 4.

1. A quantity of platina, containing its uſual admixture of magnetic duſt, was kept for ſome time of a moderate red heat in an iron ladle. The bright particles became ſomewhat duller-colour'd; the magnetic ones were no longer attracted. In other reſpects there was no ſenſible alteration.

2. An ounce of platina was urg'd with a ſtrong ſea-coal fire, in a blaſt-furnace, for above an hour: the heat was ſo vehement, that the black-lead crucible vitrify'd, and the ſlip of Windſor brick, which cover'd it, melted, and ran down. The grains of platina were found ſuperficially cohering into a lump,

of the figure of the bottom of the crucible, of a brighter colour than at firſt. On a ſlight blow, they readily fell aſunder again, and ſeem'd not to have alter'd their ſhape.

3. In ſeveral repetitions of the experiment, the platina began to cohere in a moderate white heat: the grains were at this time very eaſily ſeparable, and ſeem'd to cohere the more ſtrongly in proportion as the heat was rais'd. In the moſt intenſe fires, which the common veſſels could not long ſupport, the platina did not melt, or ſoften, or alter its figure, or loſe, ſenſibly, of its weight. The colour was conſtantly brighten'd by a ſtrong heat, and generally render'd duſky by a ſmall one: on quenching it, when violently heated, in cold water, the grains, which compos'd the internal part of the lump, acquir'd a violet or purple colour.

Experiment 5.

1. As the power of fire upon metallic, as well as earthy ſubſtances, is remarkably promoted by the immediate contact of fuel, and the impulſe of air upon the ſubject; platina was expos'd to its action in thoſe circumſtances. A crucible, having a bed of charcoal in it, was laid on its ſide, in a good blaſt-furnace, with its mouth towards the noſe of the bellows; and four ounces of platina ſpread upon the charcoal. The fire was vehemently urg'd for above an hour, during which an intenſe white flame paſs'd thro' the crucible, and iſſu'd at an aperture made for that purpoſe. The crucible was vitrify'd: the grains of platina only ſuperficially coher'd, and became brighter, as in the preceding experiment, without ſeeming to have ſoften'd or alter'd their ſhape.

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2. The experiment was ſeveral times repeated, and varied: once, common ſalt was thrown on the fuel before the crucible, and its fumes ſtrongly impell'd on the platina: ſome platina was likewiſe placed before the noſe of the bellows in violently-excited ſea-coal fires, ſo ſtrong as to almoſt inſtantly melt off a piece of the end of a forg'd iron rod, without effect; except that once there were a very few globular drops, about the ſize of very ſmall ſhot: theſe broke eaſily on the anvil, and look'd, both internally and externally, like platina.

Remark. It is probable, that the fuſion here was owing to ſome accidental admixture, poſſibly iron: for the unmelted grains, expos'd afterwards to a fire rather more intenſe, ſuffer'd no ſenſible alteration.

Experiment 6.

Platina was likewiſe expos'd to the fire in conjunction with ſeveral ſubſtances, which are found to promote the fuſion of other bodies, or to occaſion conſiderable alterations in them.

1. Platina mingled with powder'd charcoal, with compoſitions of charcoal, ſoot, common ſalt, and wood aſhes, ſubſtances employ'd for changing iron into ſteel; ſuffer'd no change in weight or appearance, whether urg'd with an intenſe fire, or cemented for many hours in a weaker one.

2. Platina was injected into melted borax, and urg'd with an intenſe fire for ſeveral hours, without undergoing any alteration. Nor had black flux, common ſalt, pure fix'd alcaline ſalts, or cauſtic alcalies, any ſenſible effect.

3. Vitreous matters were no more powerful than the ſaline. Platina was kept in ſtrong fires, for ſeveral hours, with common green glaſs, with glaſs of antimony, and with glaſs of lead, without ſeeming to be in the leaſt acted upon by either.

4. Platina was likewiſe ſtratified with plaſter of Paris, a powerful flux for the moſt difficultly-fuſible metallic body hitherto known, forg'd iron; as alſo with quicklime, and with calcin'd flint; with as little effect as in the former trials.

Experiment 7.

Nitre, which reduces all the known metallic bodies, except gold and ſilver, into a calx, was mix'd with an equal weight of platina, the mixture injected into a ſtrongly-ignited crucible, and the fire kept up for a conſiderable time; no deflagration happen'd; and the platina, freed from the ſalt by repeated ablutions with water, prov'd of the ſame weight and appearance as at firſt.

Experiment 8.

1. An ounce of platina was ſpread upon twice its weight of ſulphur, with which ſome powder'd charcoal had been previouſly mix'd to prevent its becoming fluid in the fire ſo as to ſuffer the platina to ſubſide. The crucible, having another with a hole in the bottom inverted into its mouth, was kept in a cementing furnace for ſeveral hours, when the ſulphur was found to have entirely exhal'd, leaving the platina ſeparable from the charcoal by waſhing, without alteration or diminution.

2. We

2. We likewiſe varied the experiment, injecting repeatedly pieces of ſulphur upon platina ſtrongly heated; and conſtantly found, that pure ſulphur had no more effect upon this mineral than on gold itſelf.

3. As fix'd alcaline ſalts enable ſulphur to diſſolve gold; platina was expos'd to the fire with a mixture of ſulphur and alcali, call'd *hepar ſulphuris*. After a conſiderable heat had been continu'd for ſome time, and the matter occaſionally ſtirr'd, very little of the platina was found remaining in its proper form; the greateſt part being taken up by the ſulphureo-ſaline mixture, ſo as to diſſolve along with it in water.

General Remarks.

It appears, from the foregoing experiments and obſervations,

1. That probably this mineral is originally found in large, hard maſſes, compos'd of true platina, a ſubſtance ſimilar to the black Virginia ſand, and another ferruginous matter of the emery kind, ſome ſpar, and particles of gold.

2. That theſe maſſes are, not without great labour, reduced into ſmall grains, which are afterwards ground with mercury, in order to extract the gold.

3. That the pure platina is a white metallic ſubſtance, in ſome ſmall degree malleable: that it is nearly as ponderous as gold, equally fix'd and permanent in the fire, equally indeſtructible by nitre, unaffected by ſulphur, diſſoluble by *hepar ſulphuris*. That it is not to be brought into fuſion by the greateſt degree of fire procurable in the ordinary furnaces, whether expos'd to its action in cloſe veſſels, or in contact with the fuel; by itſelf, or with the addition of inflammable, ſaline, vitreous or earthy fluxes.

II. *Ex-*

PAPER II.

Read June 20, 1754. THE more obvious properties of this extraordinary mineral, and its *habitus* to fire, ſingly, and in conjunction with the various ſubſtances call'd by the chemiſts *fluxes*, made the object of the firſt paper. In this, it is propos'd to examine the effect of acid ſpirits, ſimple and compound, applied after various manners; in order to determine not only its relation or *habitus* to them, but likewiſe its leſs obvious agreement or diſagreement with the metallic bodies, whoſe hiſtory is more known.

The platina employ'd in the following experiments was previouſly freed from its fine duſt by a ſieve; from the mercury, by ignition; and from the golden and ſome of the other heterogeneous particles, by the eye aſſiſted with glaſſes.

Experiment 1.

Platina *with the* Vitriolic Acid.

1. Several parcels of platina were digeſted for ſome hours, in a gentle heat, with ſpirit of vitriol, both concentrated, and diluted with different proportions of water. No ſolution happen'd, nor any ſenſible alteration, either in the liquors or the platina.

2. Three ounces of well-dephlegmated ſpirit of vitriol were boil'd with one ounce of platina, in a tall, narrow-neck'd glaſs, for ſome hours. The liquor remain'd nearly of the ſame quantity as at firſt; and

no

no change could be perceiv'd either in it, or in the platina.

3. The glaſs being cut off, a little above the liquid, the heat was gradually increas'd, till the liquor, which now began to evaporate, had, in five or ſix hours, totally exhal'd, and the platina become dry, and red-hot. When grown cold, waſh'd with water, and exſiccated, it was found exactly of the ſame weight as at firſt, and its grains not divided, or apparently alter'd.

Remark. Platina appears therefore to entirely reſiſt the vitriolic acid; which, by one or other of the above proceſſes, diſſolves or corrodes every other known metallic body except gold.

Experiment 2.

Platina *with the* Marine Acid.

1. Weak and ſtrong ſpirits of ſalt being digeſted, ſeparately, with one-third their weight of platina, in a gentle heat, for ſeveral hours, the liquors remain'd uncolour'd, the platina unalter'd, and undiminiſh'd. The heat was afterwards increas'd, and the liquors kept ſtrongly boiling till they had totally exhal'd, without occaſioning any ſenſible change in the platina.

2. Three ounces of a mixture of two parts decrepitated ſea-ſalt and three parts of vitriol highly calcin'd, were preſs'd ſmooth into a crucible; an ounce of platina ſpread evenly upon the ſurface, and cover'd with ſome more of the mixture; the crucible cloſely luted, and kept in a moderate red heat for ſeveral hours. On examining it when grown cold, the ſaline

mixture was found to have melted, and form'd a ſmooth, uniform lump. The platina, which had ſunk to the bottom, being ſeparated from the mixture by waſhing, prov'd of the ſame appearance as at firſt, tho' a little deficient in weight.

3. The experiment was repeated with what is call'd the *regal cement*, a leſs fuſible mixture, compos'd of common ſalt and colcothar each one part, and four parts of powder'd red bricks. An ounce of platina, ſurrounded, as above, with ſix ounces of this compoſition, and cemented in a cloſe-luted crucible with a red heat, for twenty hours, was ſtill found unalter'd in appearance, tho' there was ſome deficiency, as before, in the weight.

Remark. The marine acid, when thus detain'd in the fire by the combination of other bodies, till ſtrongly heated, and then ſet at liberty in the form of fume, diſſolves or corrodes all the known metallic ſubſtances, gold alone excepted. As the platina, in theſe experiments, retain'd its original poliſh'd ſurface, without any mark of corroſion; it was preſum'd, that this mineral likewiſe had reſiſted the marine fumes; and that the deficiency was owing to ſome of the ſmaller grains having been waſh'd off, along with the ponderous colcothar or metallic matter of the vitriol; an accident not eaſily avoided.

4. Platina was therefore treated with mercury-ſublimate, a combination of the highly-concentrated marine acid with a volatile ſubſtance, which in a proper degree of heat it readily forſakes to unite with other metallic bodies. An ounce of platina was ſpread upon three ounces of powder'd ſublimate; the glaſs cover'd, and ſet in ſand: After a moderate fire for

ſome

ſome hours, the ſublimate was found to have entirely ariſen, leaving the platina of its original weight, as well as appearance.

5. Fifty grains of a mixture of one part of platina and two of gold, well neal'd, and cautiouſly hammer'd into a thin plate, were ſurrounded with regal cement, the veſſel cover'd, cloſely luted, and kept for a conſiderable time in a red heat. Upon examining the metal, it was found to retain the whiteneſs and brittleneſs, which gold conſtantly receives from ſo large a proportion of platina; and to have loſt in weight about half a grain, or one-hundredth.

Remark. The loſs here appears to have proceeded, not from the platina, but from alloy in the gold employ'd, which was above ſtandard, but not perfectly fine: For the metal cemented a ſecond time, with freſh mixture, ſuffer'd no farther diminution. If the marine acid were capable of diſſolving platina, inſtead of one-hundredth, nearly one-third would have been exeded. This experiment therefore determines, with certainty, the reſiſtance of platina to the marine fumes; and that the regal cement, ſo call'd from its being ſuppos'd to purify gold from all heterogeneous metallic matters, is incapable of ſeparating platina from it.

Experiment 3.

Platina *with the* nitrous acid.

1. Spirit of nitre diluted with water, proof *aqua fortis*, and the ſtrong nitrous ſpirit, were digeſted ſeparately, with one-third their weight of platina, in a gentle heat, for ſeveral hours. During the digeſtion,

ſome bubbles were obſerv'd, as if a ſolution was beginning: but the liquors acquir'd no colour; and the platina, waſh'd and dry'd, was found to have neither alter'd its appearance, or loſt of its weight. The fire being afterwards increas'd, and the acid ſpirits kept ſtrongly boiling till they had entirely evaporated, no change could be obſerv'd in the platina.

2. Platina was likewiſe treated with nitrous mixtures, by proceſſes ſimilar to thoſe, in which it had been expos'd to the marine fumes. After cementation for many hours, in a red heat, with a mixture of three parts calcin'd vitriol, and two of melted nitre, the grains were recover'd not only unalter'd, but likewiſe without any deficiency in weight.

Remark. From theſe experiments it is plain, that platina, equally with gold, reſiſts the force of the vitriolic, marine, and nitrous acids, tho' apply'd in ſuch a manner, as to be capable of perfectly diſſolving all other known metallic bodies.

Experiment 4.

Platina *with* aqua regia.

1. Aqua regia, which perfectly diſſolv'd gold, pour'd upon platina, began to act on it in the cold, and, by the aſſiſtance of a moderate heat, ſlowly diſſolv'd it; acquiring at firſt a yellow colour, which deepen'd by degrees, as the menſtruum became more ſaturated, into a dark browniſh red. A few drops of the ſaturated ſolution tinged a large quantity of water of a fine golden colour.

2. The experiment was ſeveral times repeated with different aquæ regiæ, made by diſſolving ſea-ſalt and ſal

ſal ammoniac, ſeparately, in four times their weight of aqua fortis; and by abſtracting the nitrous ſpiri from the ſame proportion of each of the ſalts With all theſe menſtrua the ſolution ſeem'd to ſucceed equally.

3. In order to determine the quantity of menſtruum neceſſary for the ſolution; three ounces of an extremely ſtrong aqua regia, diluted with water, were pour'd upon one ounce of platina, in a retort, to which was adapted a recipient. A gentle heat being applied, the menſtruum acted violently, and red fumes aroſe in abundance. When about two-thirds of the liquor had come over, the action was ſcarce, if at all, ſenſible, though the fire was conſiderably rais'd. The diſtill'd liquor, which appear'd of a light rediſh colour, being pour'd back again into the retort, the ſolution began afreſh; the vapour, which now came over, appear'd pale, compar'd with the firſt. The cohobation was repeated four times, the diſtill'd liquor proving paler and paler every time: At length, both the fumes and action ceas'd, though the fire was rais'd, and a conſiderable part of the platina remain'd undiſſolv'd. The ſolution was therefore pour'd off, ſome more of the menſtruum added, the diſtillation and cohobation renew'd, and this occaſionally repeated, till the whole was taken up, excepting a little blackiſh matter, of which hereafter. The quantity of ſtrong aqua regia, employ'd for diſſolving the ounce of platina, was five ounces; but the laſt parcels appear'd from their yellow colour not to be fully ſaturated, and, upon trial, were found to take up near fifty grains of freſh platina.

Remark. It appear'd, that by this method of managing the proceſs, one part of platina was diſſoluble in about four and a half of aqua regia: But that when the digeſtion was perform'd in open veſſels in the common manner, and the fumes, which ariſe copiouſly during all metallic diſſolutions, ſuffer'd to exhale, more than half as much again of the menſtruum was requiſite. This proceſs might therefore poſſibly be applicable to advantage, in making ſolutions of metals in the way of buſineſs.

Examination of Solution of Platina.

Experiment 1.

As the vitriolic acid carries down metallic bodies, gold not excepted, from their ſolutions in other menſtrua; this acid was mix'd with ſolutions of platina.

1. When the ſolution of platina was previouſly diluted with water; the addition of dephlegmated ſpirit of vitriol occaſion'd no precipitation, or change of colour, tho' a large quantity of the acid was, at different times, dropp'd in, and the mixture ſuffer'd to ſtand for ſeveral days.

2. Dephlegmated ſpirit of vitriol, added to an undiluted ſolution of platina, immediately render'd it turbid, and threw down a duſky-colour'd precipitate. The precipitate was not re-diſſolv'd on the affuſion of water; nor was the precipitation prevented by adding water immediately after the acid had been dropp'd in.

Expe

Experiment 2.

Solutions of platina, evaporated by a gentle warmth, to a proper pitch, and then ſet to ſhoot, yielded cryſtals, of a dark, almoſt opake, red colour, in form of leaves, like flowers of benzoin, but thicker. The cryſtals, waſh'd with proof ſpirit, became ſomewhat paler, but ſtill remain'd of a high colour, reſembling the deeper chives of ſaffron. Expos'd to the fire, they ſeem'd to melt, emitted white fumes, and at length fell into a duſky aſh-colour'd calx.

Experiment 3.

Solutions of platina, dropp'd upon hot marble, immediately corroded it; but did not, like ſolutions of gold and ſome other metals, communicate any colour. Nor did they give any ſtain to the ſkin, to feathers, ivory, or other like animal ſubſtances, which liquors containing gold tinge purple.

Experiment 4.

As a minute proportion of gold contain'd in liquors is diſcoverable by their ſtriking a purple colour with tin.

1. Some bright plates of pure tin were put into a ſolution of platina diluted with water. The plates, in a little time, look'd of a dark olive colour, and ſoon after were cover'd with a rediſh brown matter: The liquor became at firſt darker colour'd, and afterwards by degrees, as the precipitate fell, nearly colourleſs; without exhibiting the leaſt appearance of a purpliſh hue.

2. Pla-

2. Platina was digeſted in a quantity of aqua regia inſufficient to diſſolve the whole; and the reſiduum diſſolv'd in a freſh parcel of the menſtruum. The two ſolutions, treated as above, yielded ſomewhat different phænomena, but no tendency to a purpliſh caſt could be perceiv'd in either. The latter, which looked yellow from not being fully ſaturated, was, when diluted with water, almoſt colourleſs. Nevertheleſs, on the addition of the tin, it became yellow again, then red, and at length of a dark browniſh red conſiderably deeper than the other more ſaturated ſolution. On ſtanding for ſome time, it grew perfectly clear, depoſiting a paler, yellowiſh precipitate.

3. To determine whether platina was capable of preventing a ſmall proportion of gold from diſcovering itſelf on this trial, one drop of a ſolution of gold was let fall into ſeveral ounces of a ſolution of platina diluted with water. On adding ſome plates of tin, the whole became immediately of a fine purple.

Remark. It may be proper to obſerve, that in theſe kinds of experiments, plates of tin are far more eligible than the ſolutions of tin uſually employ'd: For the ſolutions fail of ſtriking a purple colour with ſolution of pure gold, unleſs certain circumſtances are obſerv'd, which are not eaſily hit upon; but tin in ſubſtance conſtantly ſucceeds, and requires no particular precaution.

Experiment 5.

As gold is reviv'd from its ſolutions by inflammable ſpirits, the metal gradually ariſing to the ſurface, in form of a bright yellow cuticle;

1. A ſolution of platina was mix'd with a large proportion of highly-rectified ſpirit of wine, and expos'd for many days to the ſun, in a wide-mouth'd glaſs, ſlightly cover'd with paper, ſo as to keep out duſt. There was no appearance of any yellow ſkin; nor any other alteration, than that the platina had begun to cryſtallize from the evaporation of the fluid.

2. A drop or two of a ſolution of gold being added to a large quantity of a mixture of ſolution of platina and ſpirit of wine, and the whole expos'd as above to the ſun; a golden film was in a few days obſerv'd upon the ſurface.

Remark. It follows from this experiment, and the foregoing one with tin, that platina contains no gold; and that it cannot, any more than the common metallic or other ſoluble ſubſtances, prevent a ſmall proportion of gold mix'd with it from being diſcoverable.

Experiment 6.

1. The ſpirits of ſal ammoniac, prepar'd both by quicklime and by fix'd alcaline ſalts, added to ſolutions of platina diluted with diſtill'd water, precipitated a fine red ſparkling powder; which, exſiccated, and expos'd to the fire in an iron ladle, became blackiſh; without at all fulminating, which calces of gold, prepar'd in the ſame manner, do violently. On waſhing ſome of this precipitate upon a filter, by repeated affuſions of water, the greateſt part of it diſſolv'd; only a ſmall quantity of a blackiſh matter remaining, and the liquor paſſing through of a deep, bright, golden colour. A very large quantity of the

fluid

fluid was tinged of this colour by a ſmall one of the powder.

2. Salt of wormwood, fix'd nitre, the *lixivium ſaponarium* of the London *pharmacopœia*, precipitated a powder ſimilar to the foregoing, except that its colour was leſs brilliant.

3. Sal ammoniac likewiſe, one of the ingredients, to which the menſtruum ow'd its power of diſſolving the platina at firſt, precipitated great part of it in form of a ſimilar powder.

4. The liquors remaining after all theſe precipitations with ſaline ſubſtances, appear'd of a yellow colour, almoſt as deep as before the precipitation. Fix'd and volatile alcalies being added alternately, the liquor ſtill continu'd yellow: But either of them, added after ſal ammoniac had perform'd its office, threw down a freſh precipitate, which left the liquor colourleſs.

5. The addition of tin likewiſe, after either of the ſalts ſeparately had thrown down all they were capable of doing, occaſion'd a freſh and complete precipitation; provided a little more of the menſtruum was dropt in, to enable the liquor to act upon the metal.

Experiment 7.

As gold is totally precipitated by alcaline ſalts, but platina only in part; and as a minute portion of platina, remaining diſſolv'd, tinges a ſurpriſingly large quantity of the fluid of a yellow colour; it was preſum'd, that a ſmall admixture of platina with gold might by this means be readily diſcoverable.

A few

A few drops of a ſolution of platina were therefore mixed with above an hundred times the quantity of a ſolution of gold; the whole diluted with water; and a pure alcaline ſalt gradually added, as long as it occaſioned any effervescence or precipitation. The remaining liquor was of ſo deep a yellow colour, that it was judged the platina would have diſcovered itſelf, though its proportion had been leſs than one thouſandth part of that of the gold.

Experiment 8.

1. Zinc, which totally precipitates all the other known metallic bodies, put into a diluted ſolution of platina, was very quickly acted on, and threw down a blackiſh calx. The liquor in good meaſure preſerved its yellow colour; a mark, that part of the platina remained ſuſpended.

2. Iron, which precipitates all the metals from their ſolutions, except zinc, threw down a ſimilar calx. It could not be judged by the eye, whether the precipitation was complete, the ſolutions of iron and platina nearly agreeing with one another in colour.

3. Copper, the precipitant of mercury and gold, readily threw down platina from its ſolution, in form of a greyiſh calx, which was found upon trial to retain a notable quantity of the copper. The liquor remaining after the platina had fallen was of a more duſky green than ſolutions of pure copper, probably from its retaining ſome of the platina.

4. Mercury, which precipitates gold alone from aqua regia, put into a diluted ſolution of platina, ſeemed in a little time to be divided, and did not

run freely. Soon after, it appeared covered with a greyiſh matter, which at firſt was apprehended to be a precipitate, but was found afterwards to be a part of the mercury corroded. Upon applying a moderate heat, the whole of the quickſilver, the quantity of which was very conſiderable, was diſſolved, without any precipitation.

The experiment was repeated with a larger quantity of mercury than the ſolution was capable of taking up. The platina now gradually fell down among the undiſſolved quickſilver, in form of a dark browniſh powder; leaving the liquor nearly colourleſs.

5. A ſolution of gold mingled uniformly with a ſolution of platina, without occaſioning any turbidneſs or precipitation. The mixture, diluted with water, and ſuffered to ſtand for ſome time, threw up a bright golden pellicle to the ſurface.

Experiment 9.

1. A ſolution of platina, ſuper-impregnated with as much mercury as it was capable of taking up, on being evaporated a little, ſo as to diſpoſe it to ſhoot, yielded cryſtals not at all like thoſe of platina, but in form of ſpicula, externally of a yellowiſh hue. Theſe, ſlightly waſhed with proof ſpirit, became colourleſs: Expoſed to the fire, they emitted copious white fumes, with a hiſſing or crackling noiſe; and left a very ſmall quantity of a reddiſh powder.

2. A mixture of ſolutions of gold and platina, being treated in the ſame manner, ruby-coloured cryſtals were obtained, which appeared to be chiefly gold, with very little of the platina.

Remark.

Remark. It ſeems therefore, that mercury and gold cryſtallize from their ſolutions before platina, leaving greateſt part of that mineral diſſolved. This affair, particularly with regard to gold, deſerves farther inquiry.

Experiment 10.

As the calces of metals, obtained by precipitation from acids or by other means, vitrify along with fritt or glaſs, and tinge them of various colours; and as this proceſs is recommended by ſome for inveſtigating the nature of unknown metallic bodies; the following trials were made with precipitates of platina.

1. Half an ounce of a precipitate thrown down from ſolution of platina by plates of pure tin, was triturated in an iron mortar with eight times its quantity of common white glaſs, the mixture put into a crucible, which was cloſely luted, and placed in a wind furnace. The fire was gradually raiſed, and kept up extremely ſtrong for about ten hours; when, the crucible being taken out and broken, the matter appeared of a dark blackiſh colour, untranſparent, eaſily friable; interſperſed with a bright whitiſh matter, apparently metallic.

Remark. It is probable, that this metallic matter was the platina; and that the glaſs owed its opacity and dark colour, not to this mineral, but to the tin in the precipitate, ſome particles of iron abraded from the mortar, or other accidental cauſes.

2. A quarter of an ounce of a precipitate of platina, made by alcaline ſalt, was ground in a glaſs mortar with twelve times its weight of white glaſs; and committed to the ſame fire as the foregoing. The

reſult was a compact, cloudy glaſs, pretty tranſparent in thin pieces, covered in part with a thin whitiſh coat. Towards the upper part, and all round the ſides, were obſerved ſeveral particles of metal; which appeared to the eye like bright platina, and proved hard to the point of a knife.

Remark. Nor does the glaſs here ſeem to have received any thing from the platina; the change being no other than what white glaſs is found to undergo from a ſlight impregnation with inflammable matter.

General Remarks.

It appears from the experiments related in this paper, that platina, like gold, is not acted on by the ſimple acids, which diſſolve every known metallic body beſides: That aquæ regiæ, the ſolvents of gold, prove likewiſe menſtrua for platina: And that conſequently the common methods of aſſaying or purifying gold by *aqua fortis*, *aqua regis*, or the *regal cement*, can no longer be depended on: That it differs from gold, in giving no ſtain to the ſolid parts of animals, not ſtriking a purple colour with tin, not being revived from its ſolutions by inflammable ſpirits, not being totally precipitable by alcaline ſalts; that in certain circumſtances it throws out gold from its ſolutions; that theſe properties afford means of diſtinguiſhing a ſmall proportion of gold mixed with a large one of platina, or a ſmall proportion of platina with a large one of gold; and that platina contains no gold, excepting the few particles diſtinguiſhed by the eye: That platina is precipitated from its ſolutions by the vitriolic acid, and by the metallic ſubſtances, which precipitated gold, though ſcarce totally

totally by any: And that its precipitates reſiſt vitrification, and this perhaps in a more perfect manner than precipitates of gold itſelf.

PAPER III.

Read June 27, 1754.

THE two former papers have given an account of the *habitus* or relation of platina to the principal ſubſtances, which act upon metallic bodies; and ſhewn, that it is a ſimple metal, of a particular kind, eſſentially diſtinct from all thoſe hitherto known, though poſſeſſed of ſome properties generally ſuppoſed peculiar to gold. Many of its diſtinguiſhing characters have been already pointed out: Others will reſult from combining it with the ſeveral metals; with each of which, notwithſtanding its reſiſtance to the moſt intenſe fires by itſelf, or with unmetallic additions, it melts perfectly; occaſioning remarkable alterations in their colour, texture, and hardneſs.

ARTICLE I.

Platina *with* Tin.

1. *Equal parts* of platina and pure tin were injected into a mixture of black flux and common ſalt in ſtrong fuſion; and urged with a quick fire, in a good blaſt furnace. After a few minutes the whole appeared perfectly melted; and on being inſtantly poured out, run freely along a narrow mould, forming a ſmooth ingot, nearly of the ſame weight with the platina and tin employed. The compound proved extremely brittle, breaking eaſily from a fall:

Inter-

Internally it appeared of a close and smooth, though uneven surface; and of a dark-grey colour. By the file, or a knife, it was readily scraped into a blackish dust.

2. *One* part of platina and *two* of tin, covered with black flux, borax and common salt, were melted in a wind furnace: The platina appeared perfectly taken up by the tin, soon after the fire had been raised to a light white heat. The ingot was found deficient in weight about one-ninetieth. It greatly resembled the foregoing, being only a little less brittle, and of a somewhat lighter colour.

3. *One* ounce of platina and *four* of tin, covered with black flux and common salt, and urged with a quick fire, melted together without loss. This compound yielded a little to gentle strokes of a flat hammer, but was by no means tough. It broke in pieces from a rude blow, and was still readily scraped into dust by a knife. The surface of the fracture was rough and granulated.

4. *One* part of platina and *eight* of tin, injected into a fluid mixture of black flux and common salt, united, without loss, into a pretty tough compound; which bore to be considerably flattened under the hammer without breaking, cut smooth with a thin chissel, and shaved with a knife. Broken, it appeared of a sparkling, dark-coloured, coarse-grained texture.

5. *One* part of platina and *twelve* of tin, treated in the same manner, formed a mixture tolerably ductile; but still of a dull, dark hue, and a rough coarse grain, though somewhat less so than the preceding.

6. A mixture of *one* part of platina and *twenty-four* of tin, proved not much stiffer than tin. The colour

colour was whiter, and the grain finer and evener, than thoſe of the preceding compoſitions; though in both reſpects it fell conſiderably ſhort of pure tin.

7. Several of theſe compoſitions, covered with black flux, which had been previouſly melted, were expoſed, in crucibles cloſely luted, to a ſtrong fire in a wind furnace, which was ſteadily kept up for eight hours. When taken out, they were all found to have ſuffered ſome diminution, amounting to about one-fortieth of the tin. In appearance and quality, there was no ſenſible alteration, except that the mixture ſeemed more uniform, and the grain a little finer.

8. The remarkable gravity of platina induced us to examine the ſeveral mixtures hydroſtatically. Here it was found, that the ſpecific weight of the compound conſtantly turned out leſs than the medium of the gravities of the two ingredients; and generally the more ſo, as the proportion of the platina was the greater.

	Specific Gravity.		
	By Experiment.	By Calculation.	Difference.
Platina ——	17. 000		
Platina 1, Tin 1	10. 827	12. 090	1. 263
Platina 1, Tin 2	8. 972	10. 453	1. 481
Platina 1, Tin 4	7. 794	9. 144	1. 350
Platina 1, Tin 8	7. 705	8. 271	0. 566
Platina 1, Tin 12	7. 613	7. 935	0. 322
Platina 1, Tin 24	7. 471	7. 573	0. 102
Tin ——	7. 180		

Remarks. It appears from the foregoing experiments, that platina melts with at leaſt equal its weight of tin: That it deſtroys the malleability of near

near four times its weight: That with larger proportions it forms compounds tolerably ductile, but renders the texture of the tin coarſer, and debaſes its colour. The difference in colour of theſe compoſitions was much leſs conſpicuous on the touchſtone, than when the fractures of the ingots were examined; though, on cloſe inſpection, they appeared all ſenſibly duller and darker than pure tin, and the more ſo, in proportion as the platina prevailed. They all tarniſhed in the air; thoſe leaſt, which had a very ſmall or a very large proportion of platina.

It is remarkable, that though tin is a metal very readily deſtructible by fire, yet in moſt of the preceding fuſions, there was ſcarce any ſenſible loſs of weight. This is to be attributed not ſolely to the admixture of the platina, but likewiſe to the flux made uſe of, and more particularly to the celerity and ſhort continuance of the heat. In N° 2 and 7, the only ones, in which the loſs was at all conſiderable, the fire was ſlowly raiſed, and long continued.

ARTICLE 2.

Platina *with* Lead.

1. *Equal parts* of platina and lead were injected into a mixture of black flux and common ſalt, previouſly melted together; and the fire haſtily raiſed by bellows. A much ſtronger heat was requiſite than for the fuſion of platina with an equal quantity of tin; and the loſs was conſiderably greater, amounting to about one-ſixty-fourth. The metal yielded difficultly to the file; broke, from a moderate blow, of a cloſe texture, uneven ſurface, and rough jagged edges:

edges: The colour was very dark, with a faint purpliſh caſt.

2. *One* part of platina and *two* of lead, covered with borax and black flux, and expoſed to a gradual fire, in a wind furnace, did not come into fuſion till the fire had been raiſed to a ſtrong white heat: From the continuance of heat in this experiment the loſs was great, being nearly one-twenty-fourth of the mixture. The ingot proved hard and brittle, like the preceding, but broke off a ſtriated ſurface.

3. *One* ounce of platina and *three* of lead, treated in the ſame manner, required ſtill a very ſtrong fire for their perfect fuſion; and loſt about one-twenty-ſixth. The metal broke leſs eaſily than either of the preceding, and in ſome meaſure yielded to the hammer: The colour was ſomewhat darker, and inclined more to purpliſh.

4. *One* part of platina and *four* of lead, being covered with black flux and common ſalt, and committed to a wind furnace, the platina was not perfectly taken up, till the fire had been raiſed to a conſiderably ſtrong white heat: The loſs was one-fortieth. The ſame proportions of the metals, injected into a fluid mixture of the flux and ſalt, previouſly brought to the above degree of heat, almoſt inſtantly melted, and loſt only one-hundred-and-ſixtieth. The ingot was much tougher than of the foregoing, filed well, and cut tolerably ſmooth with a knife. Upon breaking, the upper part appeared compoſed of bright plates, the lower of dark purpliſh grains.

5. *One* part of platina and *eight* of lead united eaſily in a quick fire, and loſt little or nothing. The metal worked and looked like very bad lead: On

breaking, the texture appeared partly compoſed of tranſverſe fibres, and partly of grains; the colour dull and purpliſh.

6. *One* part of platina and *twelve* of lead united, without loſs, into a compound very little different from the foregoing. On breaking, its texture was ſomewhat finer, and compoſed chiefly of fibres, with very few grains.

7. A mixture of *one* part of platina and *twenty-four* of lead proved not very much harder than lead of a middling quality. The colour was ſtill ſomewhat purpliſh, and the texture fibrous; but the fibres were remarkably finer than where the platina was in larger proportion.

8. The foregoing compoſitions, when newly poliſhed, appeared in general of a dark iron colour; which, upon expoſure to the air, quickly tarniſhed to a browniſh yellow, a deep purpliſh, and at length a blackiſh. They all filed freely, without ſticking in the teeth of the file, as lead does by itſelf.

9. Upon returning theſe compounds to the fire a ſecond time, it was conſtantly obſerved, that after they had come into perfect fuſion, if the heat was ſlackened a little, great part of the platina ſubſided: That nevertheleſs, the lead decanted off, even in a heat below ignition, retained ſo much of the platina, as rendered it of a fine fibrous texture, and purpliſh colour.

The ſeveral mixtures, covered with black flux, and kept in ſtrong fuſion, in crucibles cloſely luted, for eight hours, ſuffered a diminution in weight, amounting to about one-thirtieth of the lead. On breaking, thoſe with a large proportion of platina appeared

peared of a leafy, and thoſe with a ſmaller, of a fine fibrous texture, which ſeemed in general to be characteriſtics of the perfect union of the platina and lead. They all looked whiter and brighter than at at firſt, but tarniſhed ſooner in the air. One mixture in particular, of four ounces of platina and twelve of lead, broke into large, white, bright, ſhining, talc-like flakes; which, on expoſure to the air, changed in a little time to a reddiſh, a purple, and a deep blue; and at length turned ſlowly to a dark blackiſh colour.

10. On examining theſe compounds hydroſtatically, their gravities turned out leſs than they ought to have been according to their calculation, but not ſo much leſs as thoſe of the compoſitions of platina and tin.

	Specific Gravity.		
	By Experiment.	By Calculation.	Difference.
Platina ——	17. 000		
Platina 1, Lead 1	14. 029	14. 193	0. 164
Platina 1, Lead 2	12. 925	13. 257	0. 332
Platina 1, Lead 4	12. 404	12. 509	0. 105
Platina 1, Lead 8	11. 947	12. 009	0. 062
Platina 1, Lead 12	11. 774	11. 818	0. 044
Platina 1, Lead 24	11. 575	11. 610	0. 035
Lead ——	11. 386		

Remark. It appears, that a ſmall proportion of platina is taken up and kept ſuſpended by lead, in a very gentle heat; but that a large proportion is not taken up near ſo eaſily as by tin, and if united by a ſtrong fire, ſubſides in part upon its abatement. A little quantity ſtiffens and hardens lead more than it

does tin; but a large one does not near ſo much diminiſh its malleability. A leafy or fibrous texture, a purpliſh colour or diſpoſition to acquire this colour in the air, are peculiar to the mixtures with lead.

ARTICLE 3.

Platina *with* Silver.

1. *Equal parts* of platina and of pure ſilver revived from *luna cornea*, covered with borax, and urged with a ſtrong fire in a blaſt furnace, melted perfectly together, and without loſs, but did not run freely along the mould. The ingot was hard to the file, and broke from a rude blow; though by gentle ſtrokes it bore to be conſiderably flattened. Internally it appeared of a much duller and darker colour than ſilver, and of a coarſer texture.

2. *One* part of platina and *two* of ſilver, covered with nitre and common ſalt, did not flow thin till the fire was raiſed to a very ſtrong white heat. The compound proved leſs brittle than the foregoing, and not ſo hard to the file: The texture was compoſed of ſmaller grains, and the colour whiter.

3. *One* part of platina and *three* of ſilver ſtill required a very ſtrong fire for their perfect fuſion: The metal was hard and brittle, though leſs ſo than the preceding: When well and repeatedly nealed, it bore to be hammered, or flattened betwixt ſteel rollers, into thin plates.

4. *One* part of platina and *ſeven* of ſilver melted together with eaſe. This compound hammered tolerably well, proved much harder than ſilver, and not ſo white, or of ſo fine a grain.

5. Theſe

5. Theſe compoſitions, weighed hydroſtatically, turned out like the others, a little lighter than by calculation: But the difference, which before ſeemed to increaſe with the platina, was here greateſt when the platina was in leaſt proportion.

	Specific Gravity.		
	By Experiment.	By Calculation.	Difference.
Platina —	17. 000		
Platina 1, Silver 1	13. 535	13. 990	0. 455
Platina 1, Silver 2	12. 452	12. 987	0. 535
Platina 1, Silver 3	11. 790	12. 485	0. 695
Platina 1, Silver 7	10. 867	11. 732	0. 865
Silver —	10. 980		

Remark. Platina appears to unite more difficultly with ſilver than with either of the foregoing metals. Even when the proportion of the platina is ſmall, the greateſt part of it ſubſides upon an abatement of the heat, by which the union had been effected. This was prevented by pouring out the metal, when perfectly fluid, at one jet, into a broad mould; in which the compound begun to congeal before the platina could ſeparate.

Platina diminiſhes the malleability of ſilver far leſs than that of tin or lead; and does not, in whatever proportion employed, ſo much debaſe its colour, or diſpoſe it to tarniſh in the air.

ARTICLE 4.

Platina *with* Gold.

1. *Equal parts* of platina and gold, expoſed to an intenſe fire, melted perfectly together, and ran thin

thin into a long mould, without loſs. The metal was of a white colour, hard to the file, broke from a rude blow, but when well nealed, yielded conſiderably to the hammer.

2. *One* part of platina and *four* of gold came into fuſion in a moderate fire, but ſtill required a very ſtrong one for their perfect union. This compound appeared but a little paler than ſtandard gold with *ſilver alloy*; and proved ſo tough, as to be beat, with proper care, into thin plates, without breaking or cracking about the edges. On melting it a ſecond time with nitre and borax, it became very pale, and was not without great difficulty made to recover its colour.

ARTICLE 5.

Platina *with* Copper.

1. *Equal parts* of platina and copper, expoſed, without addition, to a ſtrong fire haſtily excited by bellows, ſoon became fluid, but not thin; and loſt about one-ſixty-fourth. The metal proved extremely hard to the file; broke difficultly on the anvil; flew aſunder on endeavouring to cut it with a chiſſel; and appeared internally of a coarſe-grained texture and white colour.

2. *One* ounce of platina and *two* of copper, urged with a quick fire in a blaſt furnace, without addition, flowed ſufficiently thin, and ſcarce ſuffered any ſenſible loſs. The metal was ſtill very hard, and yielded but little to the hammer. It looked darker coloured than the foregoing, with a ſlight reddiſh caſt.

3. *One* ounce of platina and *four* of copper, treated in the ſame manner, united, without loſs, into a pretty

pretty tough compound; which bore to be conſiderably flattened, cut with a chiſſel, and bent almoſt double before it cracked. Internally, it looked of a fine texture, and a very pale copper colour.

4. A mixture of *one* ounce of platina and *five* of copper, ſtretched ſomewhat more eaſily under the hammer than the preceding; and appeared of a redder colour.

5. Upon increaſing the copper to *eight* times the quantity of the platina, the compound proved ſufficiently tough, broke difficultly, and hammered well. It was much harder than copper, and of a paler colour.

6. A mixture of *one* part of platina and *twelve* of copper was ſomewhat more eaſily extended under the hammer than the foregoing, and proved ſofter to the file. It ſtuck a little in the teeth of the file, which the compoſitions with a larger proportion of platina did not.

7. A mixture of *one* part of platina and *twenty-five* of copper was ſtill a little paler coloured than pure copper, and conſiderably harder and ſtiffer, though very malleable. Upon increaſing the copper a little farther, the mixture retained a degree of hardneſs, and appeared of a fine roſe colour.

8. Upon weighing the foregoing compoſitions hydroſtatically, the diminution of gravity was found more regular than in the mixtures with other metals, being conſtantly greater in proportion as the quantity of platina was larger.

	Specific Gravity. By Experiment.	By Calculation.	Difference.
Platina —	17. 000		
Platina 1, Copper 1	11. 400	12. 915	1. 515
Platina 1, Copper 2	10. 410	11. 553	1. 143
Platina 1, Copper 4	9. 908	10. 464	0. 556
Platina 1, Copper 5	9. 693	10. 191	0. 498
Platina 1, Copper 8	9. 300	9. 738	0. 438
Platina 1, Copper 12	9. 251	9. 458	0. 207
Platina 1, Copper 25	8. 970	9. 144	0. 174
Copper —	8. 830		

Remark. In the foregoing fuſions, though in general no flux was made uſe of, there was ſcarce any ſenſible loſs of weight, unleſs in N° 1, where the large proportion of platina required the fire to be raiſed to a violent degree. This ſeems owing, in good meaſure, to the platina preventing the ſcorification of the copper: For upon melting pure copper a great number of times, both with and without fluxes, there was conſtantly a little loſs.

A ſmall proportion of platina appears to improve the hardneſs of copper, without injuring its colour, or, ſo far as could be judged, its malleability. The mixtures with a large proportion of platina are difficultly extended under the hammer when cold; and whilſt red hot, fly in pieces. They all bear a good poliſh, and do not tarniſh in the air ſo much, or ſo ſoon, as pure copper.

ARTICLE 6.

Platina *with* Iron.

Iron, the laſt of the metals in point of fuſibility, was ſeveral times attempted to be united with platina, in

in its perfect malleable ſtate. But as the fluxes neceſſary for rendering forged iron fuſible corroded the crucibles before the metal flowed thin enough to diſſolve the platina, pure caſt iron was ſubſtituted.

1. Caſt iron and platina, *of each* three ounces, expoſed without addition to a ſtrong fire, united into a thick fluid; which, on adding an ounce more of iron, flowed thin. The compound ſuffered to cool in the crucible (which had become too ſoft from the heat to admit of its being poured out) was found, on breaking the veſſel, in one lump, not convex, the form, which the iron uſually aſſumes, but of a very concave ſurface: The weight about one-ſixtieth leſs than that of the metals employed. It proved exceſſively hard, ſo as not to be touched by the file; and ſo tough, as not to be broke by repeated blows of a ſledge-hammer, from which it received ſome impreſſion. Heated red, it broke eaſily, and looked internally of an uniform texture, compoſed not of bright plates like the iron at firſt, but of very dark-coloured grains.

2. *One* ounce of platina being injected upon *four* of caſt iron beginning to melt, and the fire kept up ſtrong, the whole came quickly into fuſion, and on cooling, formed an equable compound, which like the former proved extremely hard, and ſeemed to ſtretch a little under the great hammer without breaking. The colour was ſtill very dark, though leſs ſo than when the platina was in larger proportion.

3. *One* part of platina and *twelve* of iron melted without difficulty, and with little or no loſs. This compound was ſtill much harder than the iron at firſt, and had a very conſiderable degree of tough-

neſs. Like the others, it could not be broke whilſt cold, without extreme violence; but proved very brittle when heated red.

4. The foregoing compoſitions, eſpecially thoſe in which the proportion of platina was large, received a fine poliſh; and did not ruſt or tarniſh on being expoſed to the air in a dry room for ſeveral months.

5. A compoſition of one part of platina and four of iron was treated with ſubſtances, which produce notable alterations in pure iron. Surrounded with Reaumur's ſteel-making mixture (compoſed of charcoal-powder, ſoot, wood-aſhes, and common ſalt), and cemented in a cloſe luted crucible for twelve hours, it gained an increaſe of one-thirty-ninth its weight, yielded to the file more eaſily than at firſt, ſeemed to receive no additional hardneſs on being ignited and quenched in water, and diſcovered none of the qualities of ſteel. A piece broke off from the ſame ingot, treated in the ſame manner, with the powder for ſoftening caſt iron (*viz.* bone-aſh, with a ſmall proportion of charcoal), was found increaſed in weight about one-thirty-fourth, proved leſs hard to the file than at firſt, but manifeſtly harder than the part cemented with the ſteel-making mixture.

	Specific Gravity.		
	By Experiment.	By Calculation.	Difference.
Platina —	17. 000		
Platina 3, Iron 4	9. 917	11. 343	1. 426
Platina 3, Iron 12	8. 700	9. 080	0. 380
Platina 3, Iron 16	8. 202	8. 663	0. 461
Platina 3, Iron 36	7. 800	7. 862	0. 062
Iron —	7. 100		

General

General Remarks.

Platina melts with equal its weight of each of the metals; with one more readily than with another. With ſome it becomes fluid, if the proportion of the platina is not large, in a moderate fire; but a ſtrong one is conſtantly requiſite for its perfect ſolution. Compoſitions of ſilver, copper, lead, with about one-third their weight of platina, which had flowed thin enough to run freely into the mould, and appeared to the eye perfectly mixed, on being digeſted in aqua fortis till the menſtruum ceaſed to act, left ſeveral grains of platina in their original form. Upon viewing theſe with a microſcope, ſome appeared to have ſuffered no alteration; others exhibited an infinite number of minute bright globular protuberances, as if they had juſt begun to melt.

Platina hardens and ſtiffens all the metals; one more than another, lead the moſt. In a moderate quantity it diminiſhes, and in a large one deſtroys, the toughneſs of all the malleable metals; but communicates ſome degree of this quality to caſt iron. Tin bears much the leaſt, and gold and ſilver the greateſt quantity, without the loſs of their malleability.

A very ſmall proportion of platina ſcarce injures the colour of copper and gold: A larger renders both pale: A far leſs quantity has this effect upon copper than on gold. It debaſes and darkens, in proportion to its quantity, the colour of the white metals; that of ſilver much the leaſt, and of lead the moſt. It in good meaſure preſerves iron and copper from tarniſhing in the air; ſcarce alters gold or ſilver in this reſpect; makes tin tarniſh ſoon, and lead exceeding quickly.

PAPER IV.

Platina *mixed with* Semi-metals.

1. *With* Mercury.

Read July 4, 1754.

1. AN ounce of platina and ſix ounces of pure quickſilver were rubbed together, with a little common ſalt and water, and a few drops of ſpirit of ſalt, in an iron mortar. After ſome hours trituration, the grains of platina became coated with the quickſilver, ſo as to cohere into an imperfect amalgam. A part of the fluid quickſilver, poured off, and evaporated in an iron ladle, left a conſiderable quantity of a dark-coloured powder, intermingled with bright ſhining moleculæ: A part ſtrained through leather, left a ſmaller proportion of a ſimilar powder.

The platina, which had been thus attenuated by the mercury, ſo as to paſs with it through the pores of leather, proved as refractory in the fire as at firſt. Expoſed to a very vehement heat, by itſelf, with borax, with white glaſs, it neither melted, or ſuffered any ſenſible alteration; nor did it communicate any colour to either of the fluxes.

2. One part of platina and about four of lead were melted perfectly together; and after the heat had ſomewhat abated, poured gently into three times the quantity of quickſilver, heated ſo as to fume. A blackiſh powder was immediately thrown to the ſurface: This appeared to be chiefly platina. On grinding them together, a freſh powder gradually ſeparated; which, being occaſionally waſhed off, in appearance

pearance greatly reſembled the foregoing, but was found, on proper trials, to participate much more largely of the mercury and lead than of platina. The amalgam, which was of a very dull colour, on expoſure to the fire ſwelled and leapt about, though the heat was ſcarce ſufficient to evaporate the quick-ſilver. After conſtant and rapid agitation with water, occaſionally renewed, in an iron mill, for a week, it looked bright and uniform, and ſuffered the mercury to exhale freely. A dark-coloured calx remained, which proved, upon examination, to be platina, with a very little lead.

Remark. Mercury is ſuppoſed to have a greater affinity with lead than any other metallic body, gold and ſilver excepted. In this experiment, it had a greater affinity with platina than with lead, ſince it retained moſt of the platina, after the lead, which was in much larger proportion, had been almoſt intirely thrown out. The part of the platina, which the mercury rejected at firſt, and that which it retained to the laſt, did not appear diſſimilar to one another, or different in quality from the platina employed.

3. A mixture of one part of platina and two of gold, which proved very white and brittle, after being repeatedly nealed, was cautiouſly flattened into thin plates, and thrown red-hot into boiling quick-ſilver. On trituration and ablution with water, a powder ſeparated, copiouſly at firſt, and by degrees more ſparingly. After the proceſs had been continued about twenty-four hours, there was no farther ſeparation, except of a very little blackiſh matter, into which a part of the mercury is always changed in theſe

kinds

kinds of operations. The amalgam, which looked very bright, left, upon evaporation, a ſpongy maſs, of a high colour, which being melted, and poured into an ingot, proved very ſoft, extremely malleable, and in all reſpects reſembled the pure gold made uſe of, without the leaſt appearance of platina.

Remark. It is greatly to be wiſhed, that this method of purifying gold from platina may prove ſufficiently accurate to exactly determine the quantity of each in the mixt. The experiments hitherto made do not ſufficiently clear up this point; a great number are ſtill neceſſary before it can be fully aſcertained.

2. *With* Biſmuth.

Equal parts of platina and biſmuth, injected into a mixture of black flux and common ſalt, previouſly brought into fuſion, and urged with a quick fire, ſtrongly excited by bellows, melted perfectly in a few minutes, and ſuffered very little loſs. Without theſe precautions, the biſmuth could ſcarce be made to take up above one-third its weight; great part of which, on an abatement of the heat, ſubſided.

Mixtures of platina with different proportions of biſmuth proved all, like the biſmuth itſelf, extremely brittle: One was not remarkably more ſo than another. To the file, they were ſcarce harder than pure biſmuth. They broke of an irregular ſurface, compoſed chiefly of ſtriæ, with ſome plates. When newly broken, they looked bright and ſparkling; except the compoſitions with a large proportion of platina, which were of a dull greyiſh colour, without any brightneſs. They all tarniſhed ſlowly in the air,

air, to a dark yellowiſh, purpliſh or bluiſh colour: Several acquired in part a fine deep blue, which has ſuffered no change in above a twelve-month; ſome parts of the maſſes ſtill remaining white as at firſt, and others inclining to purple.

3. *With* Zinc.

Upon an ounce of platina, covered with borax, and heated in a blaſt furnace to a ſtrong white heat, was injected an equal quantity of zinc. A violent deflagration aroſe, and the platina was almoſt inſtantly diſſolved: The matter, immediately poured out, was found to have loſt near half an ounce.

Upon ſeveral times repeating this experiment with different proportions of the two metals, both in a quick fire, and in one more gradually raiſed in a wind furnace, the zinc was conſtantly found a powerful menſtruum for platina, but ſuffered great loſs from the heat requiſite for rendering the mixture ſufficiently fluid. When ſo much of the zinc had been diſſipated, that the remainder amounted to no more than one-fourth of the platina, the compound ſtill continued fluid enough to run freely into a long mould.

Compoſitions of platina and zinc differed little in appearance from zinc itſelf; except that where the quantity of platina was large, they were of a cloſer texture, and a duller hue, with rather more of a bluiſh caſt. They did not tarniſh, or change their colour, on being expoſed for ſeveral months to the air, in a dry room. They were harder to the file than the zinc at firſt, and fell in pieces under the hammer;

hammer, without at all ſtretching; which pure zinc does in a conſiderable degree.

4. *With* Regulus of Antimony.

Regulus of antimony, the moſt difficultly fuſible of the ſemi-metals, diſſolved, in a ſtrong fire, equal its weight of platina. The compound looked of a much duller colour than the regulus at firſt; and broke of a cloſe and uniform, though uneven, ſurface. It proved conſiderably harder to the file, but not remarkably more or leſs brittle.

On increaſing the quantity of the regulus, the compound proved brighter, and of a leafy texture, little different from that of the pure regulus.

Platina *mixed with* Compound Metals.

1. *With* Braſs.

1. Equal parts of platina and braſs, covered with borax, and urged with a quick fire in a blaſt furnace, melted perfectly together, and ſcarce ſuffered any loſs. The mixt was of a greyiſh white colour, filed hard like bell-metal, broke from a blow of the hammer, without ſtretching or receiving any impreſſion, and flew aſunder on endeavouring to cut it with a chiſſel. Internally, it appeared of an uniform fine grain, a cloſe texture, and a darker colour than on the outſide. It bore a very fine poliſh, and did not tarniſh on being expoſed to the air in a dry room for many months.

2. One part of platina and two of braſs, melted in a ſlow fire, loſt about one-thirty-ſixth. The ingot was of a duller colour than the foregoing, with a faint

a faint yellowiſh caſt: It filed ſofter, broke leſs readily from the chiſſel, but cracked and fell in pieces under the hammer.

3. One part of platina and four of braſs, covered as before with borax, and expoſed to a quick fire, melted without loſs. This compound proved yellower than the preceding, filed ſofter, bore to be cut ſome depth with a chiſſel before it broke, and received ſome impreſſion from the hammer, ſtretching a little, but ſoon cracking in various directions.

4. Upon increaſing the braſs to ſix times the weight of the platina, the compound appeared yellower, though ſtill very pale. It proved ſofter to the file; and received a greater impreſſion from the hammer, and a deeper one from the chiſſel, before it broke.

5. A mixture of one part of platina and twelve of braſs was conſiderably paler, and much harder, than braſs. It broke from the chiſſel; and cracked, before it had extended much, under the hammer. It bore a good poliſh, and was leſs apt to tarniſh than braſs; though in both reſpects it fell ſhort of the compoſitions with larger proportions of platina.

2. *With* Copper *and* Tin.

1. One hundred parts of platina, thirty-four of copper, and twelve of tin, covered with borax, became fluid in a ſtrong fire, and ſuffered no conſiderable loſs. The ingot proved extremely hard, ſo as ſcarce to be touched by the file; and very brittle, breaking from a moderate blow, of a rough ſurface, and dull bell-metal colour. It bore a good poliſh, and did not tarniſh in the air.

2. Platina and copper, of each one ounce, and four ounces of tin, melted perfectly together, and without loſs. This compound filed freely and eaſily, bore to be cut with a knife, but broke readily on the anvil, of an irregular ſurface, and dull whitiſh colour. Poliſhed, it looked like poliſhed iron. The fracture ſoon tarniſhed to a yellow; the poliſhed part grew dull, but retained its colour.

3. A mixture of platina and copper, of each one part, and eight of tin, proved ſofter than the foregoing; and bore to be flattened a little under the hammer. It broke of a very irregular ſurface, compoſed of a great number of bright white plates. The fracture ſoon tarniſhed; the poliſhed part retained its colour.

Remark. It is obſervable, that in the firſt of theſe experiments, platina was perfectly taken up by leſs than half its weight of a mixture of copper and tin; though it could ſcarce be made to melt with leſs than its own weight of either of them ſeparately, in a fire equally, or rather more, intenſe.

The ſpecific gravity of theſe mixtures turned out, upon experiment, a little leſs than by calculation; though the copper and tin, melted together without platina, formed a compound ſpecifically heavier than even the copper by itſelf.

The ſeveral mixtures with zinc, biſmuth, regulus of antimony and braſs, were likewiſe weighed hydroſtatically, and found all ſomewhat lighter than they ought to have been by calculation. As few hydroſtatical experiments ſeem to have been made upon zinc and biſmuth, it may be proper to mention, that

that the gravity of pure zinc turned out 7. 050, and that of biſmuth 9. 733.

Hitherto we have conſidered the miſcibility of platina with metallic bodies, and the alterations, which different proportions of it produce in their appearance and qualities: employing the neceſſary precautions for preventing the ſcorification and diſſipation, which moſt of the metals ſuffer in the fire; and which ſome remarkably promote in thoſe, which by themſelves are more difficultly, or not at all, deſtructible. We ſhall now examine the relation of platina, in this reſpect, to thoſe metallic ſubſtances, which are the moſt deſtructive.

1. *Cupellation and Scorification of* Lead *with* Platina.

1. A mixture of platina and lead was cupelled, under a muffle, in an aſſay-furnace. For ſome time the proceſs went on well; the lead gradually changing into ſcoriæ, which were thrown of to the ſides, and abſorbed by the cupel, or diſſipated in fume. In proportion as the lead worked off, the matter required a ſtronger fire to keep it fluid; and at length, collecting into a dull flat lump, could no longer be made to flow in the greateſt degree of heat, which the furnace was capable of giving. The lump broke very eaſily, appeared of a dull grey colour both internally and externally, and of a porous texture. It weighed about one-fifth more than the quantity of platina employed.

2. This experiment was many times repeated and varied: the lead attempted to be worked off on bone-aſh, preſſed into the bottoms of crucibles, ſco-

rified in aſſay-crucibles, by intenſe fires, in a blaſt furnace, and blown off on teſts before the noſe of a bellows, with the ſame event; the platina not only perfectly reſiſting the power of lead, which by theſe operations deſtroys every other known metallic body, except gold and ſilver, but likewiſe retaining and preventing the ſcorification of a part of the lead itſelf.

3. In the hiſtory of the fuſion of platina with lead, it has been obſerved, that this metal depoſites in a gentle heat great part of the platina, which had been united with it by a ſtrong one. As the part, which remained ſuſpended, might be preſumed to differ from that, which ſubſided; a quantity of lead was decanted off from freſh parcels of platina, and both the decanted metal and the reſiduum ſubmitted to the preceding operations ſeparately. The event was ſtill the ſame; the matter becoming conſiſtent, when the lead had been worked off to a certain point, and refuſing farther ſcorification.

4. A mixture of platina and lead, which had been cupelled in an aſſay-furnace as long as it could be kept fluid, was expoſed in a crucible to a fire vehemently excited, by itſelf, with powdered charcoal, with black flux, borax, nitre, common ſalt. The matter neither melted, or ſuffered any conſiderable alteration, becoming only ſomewhat more porous; probably from a little of the lead having exſuded without the liquefaction of the maſs. The immediate contact of burning fuel, agitated by bellows, made ſome of theſe mixtures flow, after they had refuſed to melt in veſſels acted upon by intenſe fires. Very little of the lead was diſſipated by this means.

5. Upon

5. Upon examining the cupelled matters hydroſtatically, thoſe, which appeared moſt ſpongy, were found nearly as ponderous as the crude platina. Among the more compact, the gravity of one turned out 19.083; of another 19.136, and of a third 19.240.

Remark. It appears from theſe experiments, that platina, like gold and ſilver, is intirely indeſtructible by lead; that probably the purer grains, or fragments, have ſome heterogeneous admixtures, which are ſeparated in theſe operations; and that, perfectly pure, it is more ponderous than gold, ſince, when mixed with a conſiderable proportion of a lighter metal, it fell very little ſhort of the gravity of pure gold. There is no reaſon to ſuſpect any increaſe of its ſpecific gravity from the mixture; ſince in all the compoſitions with platina hitherto examined, there was conſtantly a diminution of the ſpecific gravity; whether the proportion of the platina was large or ſmall, the matter melted with a quick fire, or kept in fuſion for many hours.

6. A mixture of one part of platina and three of gold was cupelled with lead, under a muffle. The matter drove well for a conſiderable time; at length collected into a bright hemiſpherical lump, which by degrees became flatter, dull, and rough. The button, on being weighed, was found to retain a conſiderable portion of lead.

The experiment being repeated with a mixture of one part of platina and ſix of gold, ſome part of the lead was ſtill retained. The bead proved rounder and brighter than the foregoing, and of a good golden colour on the outſide: It broke eaſily under the hammer, and appeared internally greyiſh: Some of the fragments

fragments hung together by the outward golden coat.

7. Mixtures of platina and ſilver, ſubmitted to cupellation, retained likewiſe a conſiderable quantity of the lead. Theſe, in becoming conſiſtent, formed, not an hemiſpherical bead, but a flat maſs, very rough, and brittle, and of a dull grey colour both internally and externally.

2. *Cupellation and Scorification of* Biſmuth *with* Platina.

Mixtures of platina with biſmuth, a metallic ſubſtance, in ſome reſpects more active than lead, were cupelled under a muffle, ſcorified in aſſay-crucibles, teſted before the noſe of a bellows. In numerous repetitions of theſe experiments, the event was the ſame, as when lead was made uſe of. The mixtures, which at firſt flowed eaſily, became leſs and leſs fuſible, in proportion as the biſmuth was driven off; and at length could not be kept fluid in an intenſe white heat, though they appeared, on weighing, to retain a conſiderable proportion of the biſmuth. Nor could this ſemi-metal, any more than lead, be intirely ſeparated, by cupellation, from mixtures of platina with either gold or ſilver.

Platina cupelled with biſmuth, differed little in appearance from that, which had been treated in the ſame manner with lead. The button was more ſpongy, and ſpecifically lighter.

3. *Diffiation of* Regulus of Antimony *with* Platina.

A mixture of platina and regulus of antimony was melted, by a ſtrong fire, in a ſhallow wide crucible, and

and the noſe of a bellows directed obliquely upon the ſurface. The matter continued to flow, and fume copiouſly, for ſome hours; at length became conſiſtent in an intenſe white heat, and ſcarce emitted any more fumes, though ſtrongly blown on. The maſs, when grown cold, broke eaſily, appeared very porous, blebby, of a dull grey colour, and weighed conſiderably more than the quantity of platina employed.

Platina was likewiſe treated with crude antimony; and the regulus, obtained from this mixture, difflated as the foregoing, with the ſame event; the platina not only reſiſting the antimonial ſemi-metal, but likewiſe defending a part of it from the action of the fire and air, and refuſing to melt, after a certain quantity had been diſſipated.

4. *Deflagration of* Zinc *with* Platina.

A mixture of platina and zinc, expoſed to a ſtrong fire, deflagrated, and appeared in violent agitation. This continued but a little time; the matter quickly became ſolid, and could no longer be made to flow; or the zinc, of which a conſiderable proportion remained in it, to flame. The maſs was very brittle, dull-coloured, ſpongy, and of no ſpecific gravity.

General Remarks.

This extraordinary mineral, on which the moſt active fluxes, aſſiſted by the moſt intenſe fires, have no effect, melts perfectly with all the known metallic bodies; unleſs arſenic, a ſubſtance impatient of a degree of heat ſufficient to render itſelf fluid, is an exception. All the metals take up equal their own weight;

weight; ſome metallic compoſitions more than twice their weight.

Platina appears in general to have no remarkable affinity with one metal more than with another. Lead and iron, which do not mingle together, and of which the former will take up ſome bodies from the latter, and the latter ſome from the former, ſeem both indifferent to platina; which, if combined with either, is not ſeparated by the other.

Nevertheleſs ſome ſubſtances have greater or leſs degrees of affinity with platina than with other metallic bodies. Thus, from aqua regia, in certain circumſtances, it throws out gold; and is itſelf precipitated by the other metals, which diſſolve in that menſtruum. From quickſilver it throws out lead; and is itſelf thrown out by gold.

The changes, which platina occaſions in the perfect metals, were examined in a former paper: Its effects on the ſemi-metals are leſs remarkable. The principal are, that it increaſes the hardneſs of zinc, and the antimonial ſemi-metal, but not of biſmuth; and diſpoſes this laſt to change its colour in the air, but not the others.

Its effects on the compound metals are ſimilar to thoſe, which it produces on the ſimple ones. Braſs it renders white, hard, brittle, ſuſceptible of a fine poliſh, and not liable to tarniſh in the air, as it does the copper, and in ſome degree the zinc, of which this metal is compoſed. Mixtures of it with copper and tin are more apt to tarniſh than with copper only, and leſs than with tin only.

All metallic ſubſtances, except gold, are exeded from platina by the ſimple acids: Mercury is the only

one

one ſeparable by fire. The platina remaining after the ſeparation of the metals, proves unfuſible as at firſt.

Platina perfectly reſiſts the deſtructive power of lead and biſmuth, and the rapacious antimonial ſemimetal; which laſt has been hitherto eſteemed the ſevereſt teſt of gold, ſo as to have received the appellation of *balneum ſolius ſolis*, the bath which gold alone can ſuſtain, and in which it is waſhed from all kinds of impurities.

Since therefore platina mixed with gold is not diſcoverable by any of the operations by which that metal is uſually aſſayed or refined, or by the hydroſtatic balance; we hope that theſe papers, which contain part of the hiſtory of this extraordinary and hitherto unknown mineral, and the methods of diſtinguiſhing any ſophiſtications of gold made by its means, which might otherwiſe have paſſed undiſcovered, will be candidly received by this illuſtrious body, as a means of promoting that kind of knowlege, for which the Royal Society has been ever eminent, and peculiarly inſtituted.

LXXXVII. *An Explication of all the Inſcriptions in the* Palmyrene *Language and Character hitherto publiſh'd. In five Letters from the Reverend Mr.* John Swinton, *M. A. of* Chriſt-Church, Oxford, *and F. R. S. to the Reverend* Thomas Birch, *D. D. Secret. R. S.*

LETTER I.

SIR, Chriſt-Church, Oxford, May 30, 1754.

Read June 20, 1754. SEveral copies of the *Ruins of Palmyra* reached Oxford, towards the cloſe of December 1753; one of which was purchaſed by the Reverend Mr. Godwyn, Fellow of Balliol College, a gentleman of great learning, and an eminent tutor of that houſe. At his invitation, I looked over with him the finiſhed plates, exhibiting to our view thoſe noble remains of antiquity, which gave both of us infinite pleaſure and delight; though we then only caſt our eyes upon the inſcriptions, and particularly thoſe that are the ſubject of this letter, in a curſory manner.

In the beginning of January 1754, Mr. Godwyn informed me, that he had diſcovered ſeveral letters of the Palmyrene alphabet, by the help of the Greek inſcriptions correſponding with ſome of thoſe drawn in the Palmyrene character; and that he could even decipher a few words in ſeveral of the latter inſcriptions. At the ſame time, he deſired me to apply myſelf to the interpretation of thoſe inſcriptions, and the

diſcovery

NB. The Arabic figures added to the Roman numerals refer to th

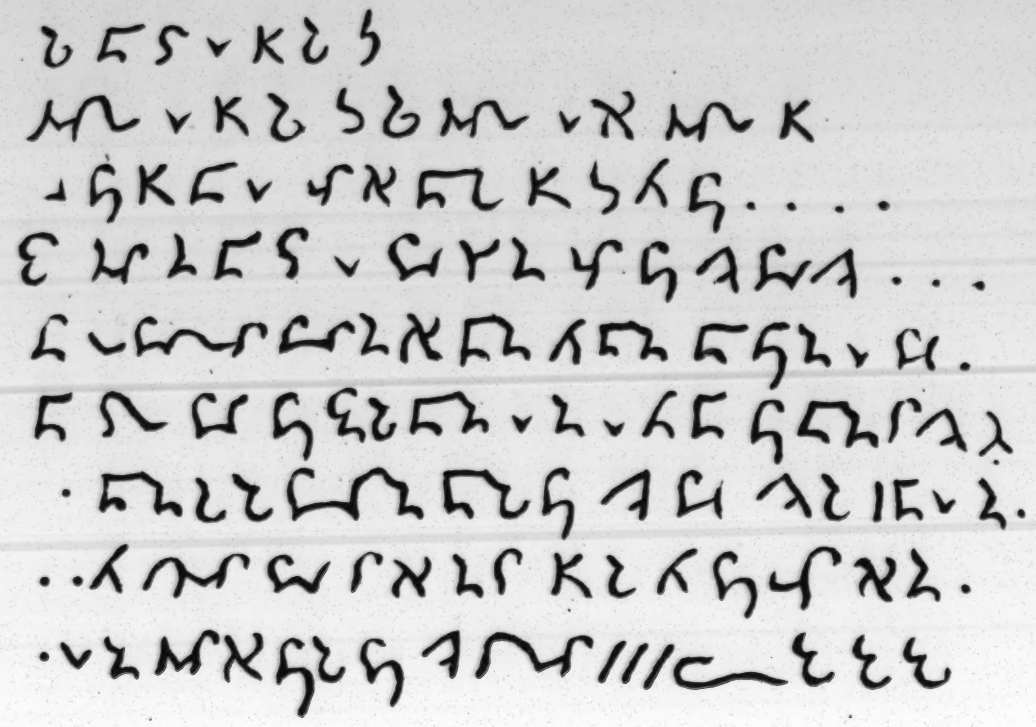
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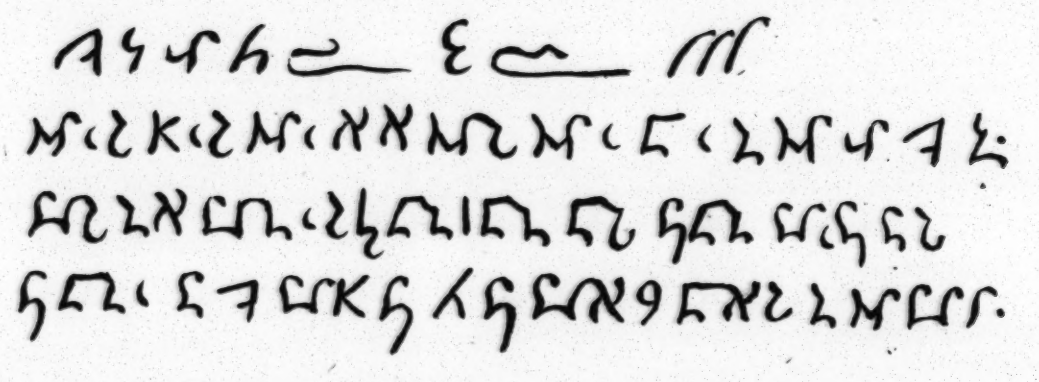
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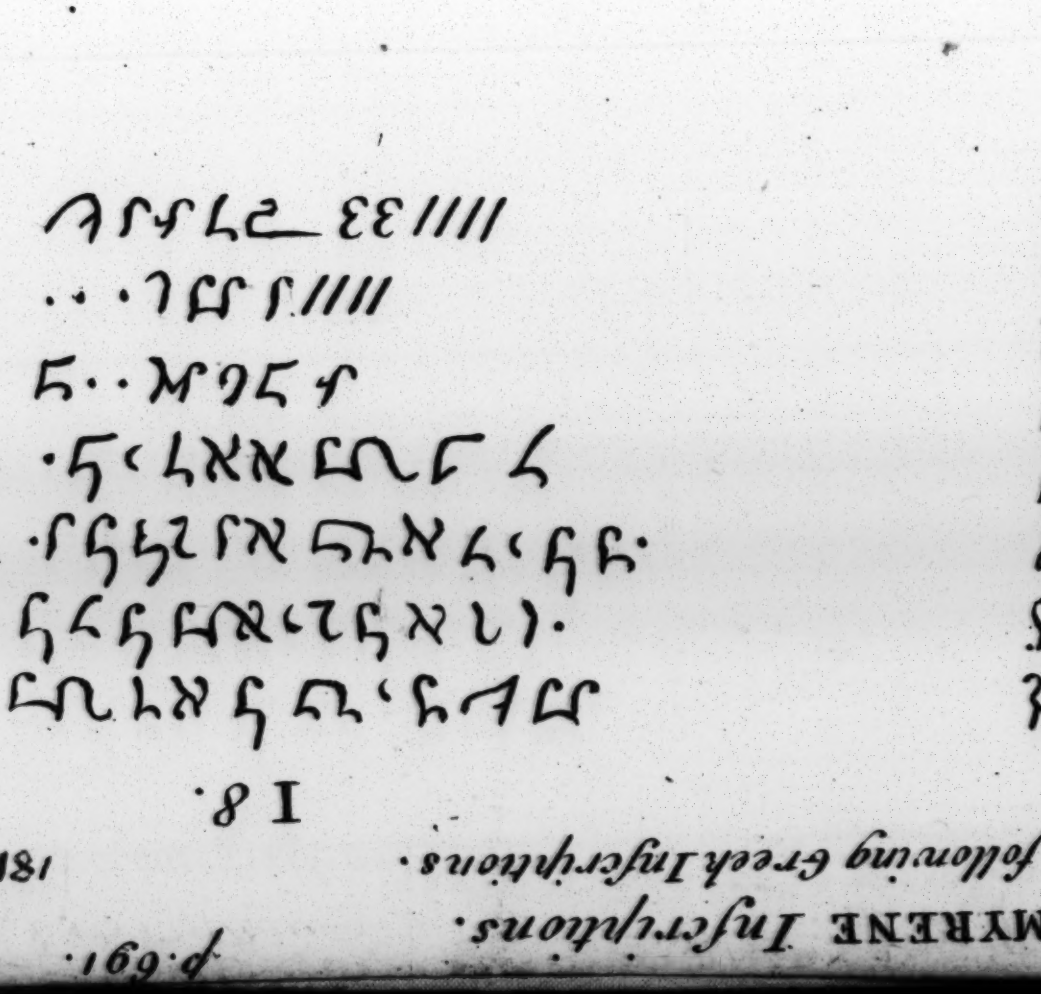
J. Myndc fculp.



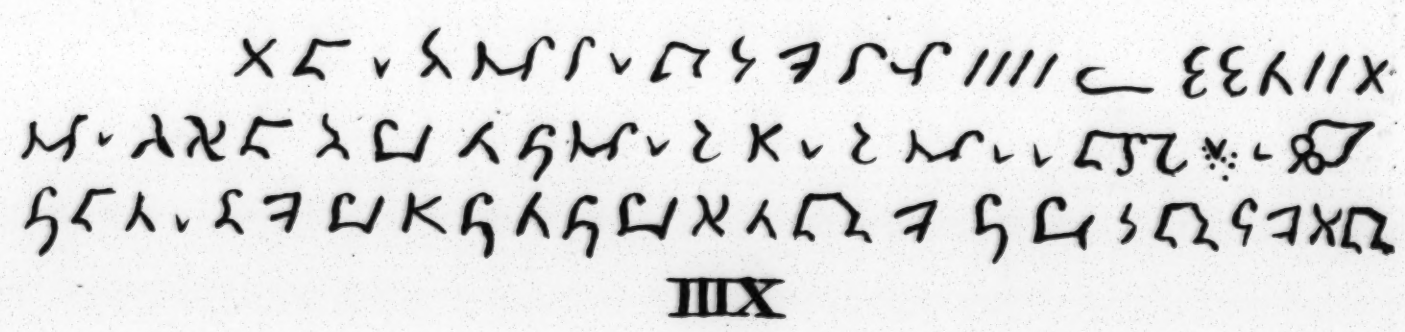
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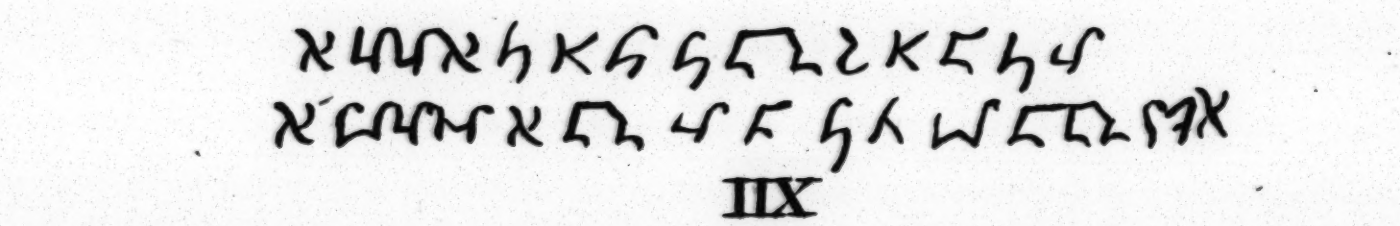
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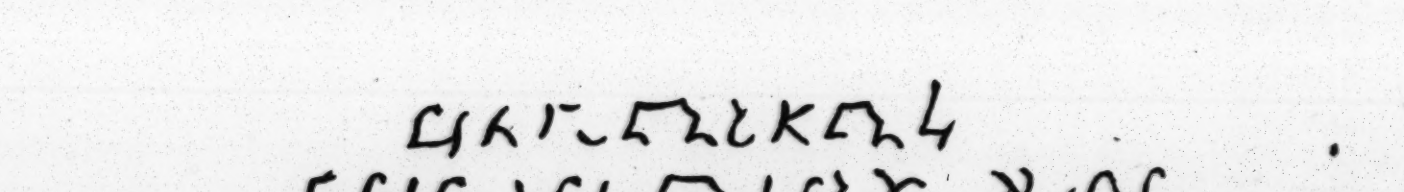
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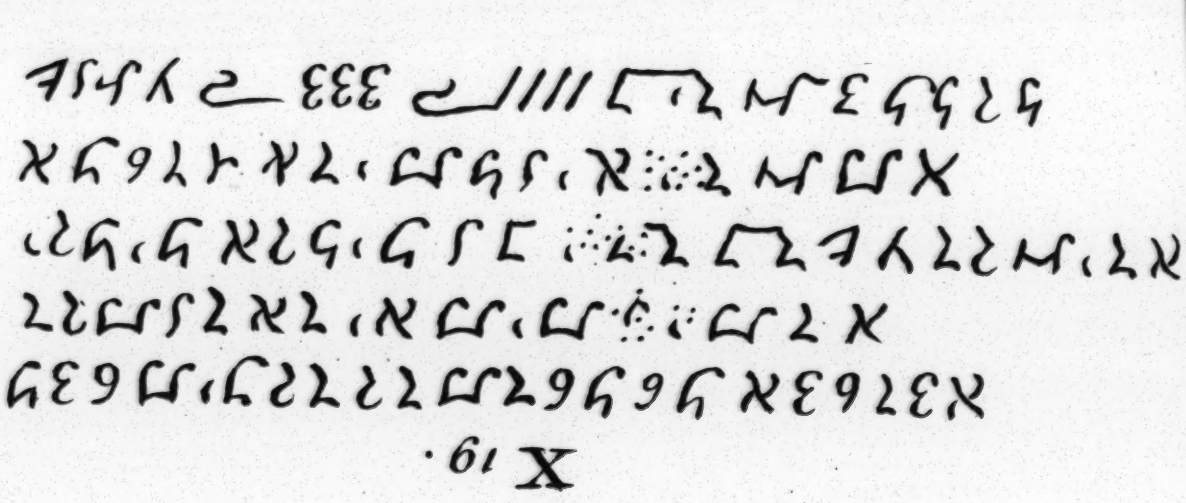
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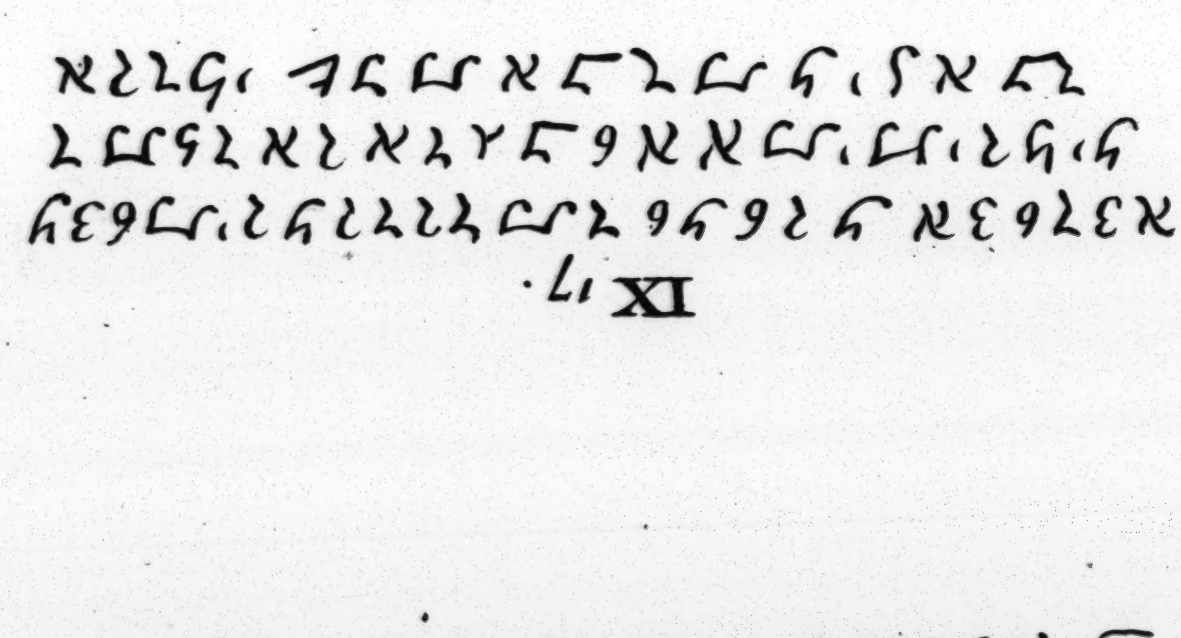


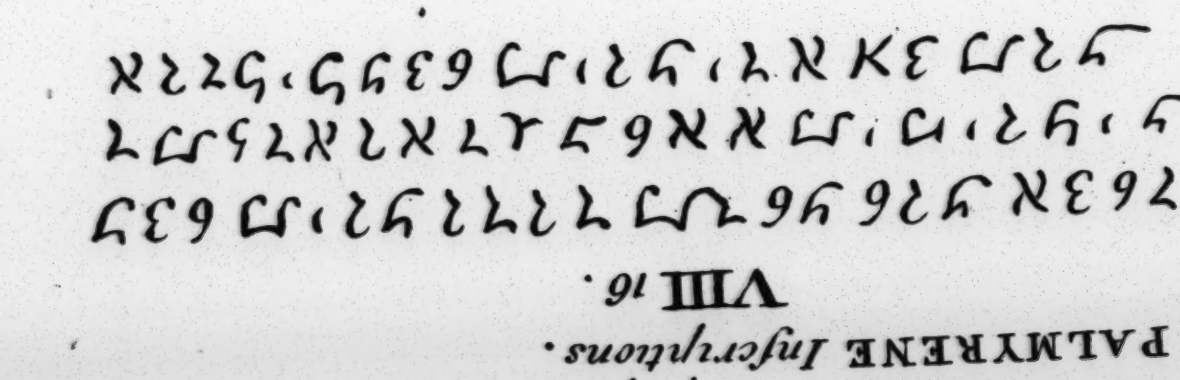
XI

Philos. Trans. Vol. XLVIII. TAB. XXV. From Mr. Dairhins's plate of P

J. Myndc fculp.







p. 691.

I

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III

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ΕΝΙΑΜΛΙΧΟCΜΟΚΕΙΜΟΥΤΟΥΚΑΙΑΚΚΑΛΕΙCΟΥ
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ΑΥΔΥΝΑΙΟΥΚΔ

J. Mynde sculp.

IX

ΗΒΟΥΛΗΚΑΙΟΔΗΜΟϹ
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X

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XI

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XII

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$\bar{\text{Σ}}\bar{\text{Ζ}}\bar{\Phi}$ ΥΠΕΡΒΕΒΕΤΑΙ

XIII

ΗΒΟΥΛ
ΑΥΡΗΛΙΟ
ΜΑΛΗ
ΑΝΑΚΟΜΙϹ
ΠΡΟΙΚΑ$\bar{\text{Ε}}\bar{\text{Ζ}}$ΙΔΩΝΤ
ΕΤΟ$\bar{\text{Θ}}\bar{\text{Ζ}}\bar{\Phi}$

olate of GREEK *Inscriptions*.

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NAIPANHNO
YTONΛAMΠPO
IKΛHTIKON

XII

NΩN
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KHCTON
· · ΘIMHCKAIEYXA
APINETOYC Γ̄ Σ̄ Φ̄

XII

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CEΛEY
CEEIΛA
ΩC
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TEIMHC
PATICTH
MYPIAC
VEKENETOYC
ΩBEETAIΩ

XIII

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ΣYNOΔIAN
IC
Σ̄ IΔΩN TEIMHCXAPIN
ETOC Θ̄ Ξ̄ Φ̄

J. Mynde sculp.

Philos. Trans. Vol. XLVIII. TAB. XXVIII. *From Mr. Dawkins's* II. *plate of* GREEK *Inscriptions.*

XIV

.ΥΠΙΛΙΟΝΟΥΟΡΩΔΗΝ
ΙΠΠΙΚΟΝΚΑΙΒΟΥΛΕΥΤΗΝ
ΠΑΛΜΥΡΗΝΟΝΒΗΛΑ
ΚΑΒΟCΑΡCΑΤΟΝΦΙ
. . . .ΕΙΜΗCΧΑΡΙΝ
ΕΤΟΥC Ō Φ̄

XV

ΕΠΤΙΜ
ΤΟΝΚΡΑ
ΠΟΝCΕΒ H
ΝΑΡΙΟΝΚΑΙ ,HN
ΙΟΥΛΙΟCΑΥΡ OCCE

XVII

ΕΠΤΙΜΙΟΝΟΥΟΡΩΔΗΝ
ΤΟΝΚΡΑΤΙCΤΟΝΕΠΙΤΡΟ
ΠΟΝCΕΒΑCΤΟΥΔΟΥΚΗ
ΝΑΡΙΟΝΚΑΙΑΡΓΑΠΕΤΗΝ
ΙΟΥΛΙΟCΑΥΡΗΛΙΟCCΑΛΜΗC
ΚΑCCΙΑΝΟΥΤΟΥ^ ·· ΕΝΑΙΟΥ
ΙΠΠΕΥCΡΩΜΑΙΩΝΤΟΝΦΙΛΟΝ
ΚΑΙΠΡΟCΤΑΤΗΝΕΤΟΥCΗ Ō Φ̄
ΜΗΝΕΙΖΑΝΔΙΚΩ

185

ΠΟΝCΕΒ Η
ΝΑΡΙΟΝΚΑΙ ,ΗΝ
ΙΟΥΛΙΟCΑΥΡ ΟCCΕ
ΠΥΙΩΟCΜ ΜΑΛΩ
ΚΑΝΝΑCCΟΥ ΟΚΡΑΤΙ
CΤΟCΤΟΝΦΙΛΟΝΚΑΙΠΡΟCΤ
ΑΤΗΝΤΕΙΜΗCΕΝΕΚΕΝ
ΕΤΟΥCΖΟΦΩΗΝΕΙΖΑΝΔΙΚΩ

XVI

CΕΠΤ ΟΥΟΡΩΔΗΝ
ΤΟΝΚΡΑΤΙCΤΟΝΕΠΙΤΡΟ
ΠΟΝCΕΒΑCΤΟΥΔΟΥΚΗ
ΝΑΡΙΟΝΚΑΙΑΡΓΑΠΕΤΗΝ
ΙΟΥΛΙΟCΛΥΡΗΛΙΟC
CΕΠΤΙΜΙΟCΙΑΔΗCΙΠ
ΠΙΚΟCCΕΠΤΙΜΙΟΥΑΛΕ
ΞΑΝΔΡΟΥΤΟΥΗΡΩΔΟΥ
ΛΠΟCΤΡΑΤΙΩΝΤΟΝΦΙ
ΛΟΝΚΑΙΠΡΟCΤΑΤΗΝ
ΤΕΙΜΗCΕΝΕΚΕΝΕΤΟΥC
ΗΟΦΜΗΝΕΙΖΑΝΔΙΚΩ

XVIII

ΗΒΟΥ ΜΟC
CΕΠΤΙΛ ΤΟΝΚΡΑ
ΤΙCΤΟΝC ΕΒΑCΤΟΥ
ΔΟΥΚΗΝ ΕΟΔΟΤΗΝ
ΤΗCΜΗ ΝΕΙΑCΚΑΙΑ
ΝΑΚΟΜΙC CCΥΝΟΔΙΑC
ΕΞΙΔΙΩΝΚΑΙΜΑΡΤΥΡΗΘΕΝΤΑ
ΥΠΟΤΩΝΑΡΧΕΜΠΟΡΩΝΚΑΙ
ΛΑΜΠΡΩCCΤΡΑΤΗΓΗCΑΝΤΑΚΑΙ
ΑΓΟΡΑΝΟΜΗCΑΝΤΑΤΗCΑΥΤΗC
ΜΗΤΡΟΚΟΛΩΝΕΙΑCΚΑΙΠΛΕΙCΤΑ
ΟΙΚΟΘΕΝΑΝΑΛΩCΑΝΤΑΚΑΙΑΡΕCΑΝ
ΤΑΤΗΤΕΑΥΤΗΒΟΥΛΗΚΑΙΤΩΔΗΜΩ
ΚΑΙΝΥΝΕΙΛΑΜΠΡΩCCΥΜΠΟCΙΑΡ
ΧΟΝΤΩΝΤΟΥ ΔΙΟCΒΗΛΟΥΙΕ
·ΩΝ ΤΕΙΜΗCΕΝΕΚΕΝ
Ζ·ΑΝ

J. Mynde sculp.

XIX

·ЄΠΤΙ
ЄΠΙΤΡΟΠ ΟΥΚΗΝΑΡΙΟΝ
ΙΟΥΛΙΟCΑΥΡΗ Ο ΒΑΛΟC · ·
ΔΟΥΤΟΥ ΗΓΟCΙ·C ΛΑΜΠΡΟΤΑ
ΤΗCΚΟΛΩΝΕΙΑC·ΟΝΕΑΥΤΟΥ ΦΙΛΟΝ
ΤΕΙΜΗCΕΝΕΚΕΝ ΕΤΟΥC · · · ΜΗΝΕΙ
ΛΠΕΜΑΙΩ ,

XX

· · · · · · · · · ·ΟΔΗΜΟC
ΜΑΛΗΝΤΟΝΚΑΙΑΓΡΙΠΠΑΝ
ΙΑΡΑΙΟΥΤΟΥΡΑΑΙΟΥΓΡΑΜΜΑ
ΤΕΑΓΕΝΟΜΕΝΟΝΤΟΔΕΥΤΕ
ΡΟΝΕΠΙΔΗΜ · · ΘΕΟΥ ΑΔΡΙ
ΛΝΟΥΑΛΛΙΜ ΜΑΠΑΡΑCΧΟ
ΤΑΞΕΝΟΙCΤΕΚΑΙΠΟΛΕΙΤΑ · ·

XXI

ΗΒΟΥΛΗΚΑΙΟΔΗΜΟCΒΑΡΕΙΧΕΙΝ
ΑΜΡΙC·ΜCΟΥΤΟΥΙΑΡΙΒΩΛΕΟΥC
ΚΑΙΜΟΚΙΜΟΝΥΙΟΝΛΥΤΟΥΕΥΕCΒΕΙC
ΚΑΙΦΙΛΟΠΑΤΡΙΔΑCΤΕΙΜΗCΧΑΡΙΝ

XXII

ΜΑΛΙΧΟΝΝΕCΑΤΟΥΚΩΜ·ΑΤΟΥΕΠΙΚΛΛΟ
ΥΜΕΝΟΥΑCΙΑCΟΥΦΥΛΗCΧΩΜΑΡΗΝΩΝΠΑΛ
ΜΥΡΗΝΩΝΟΔΗΜΟCΕΥΝΟΙΑCΕΝΕΚΑ

XXIII

ΓΕΗΔΑΤΙΟΝΟΥΕΛΛΗΙΟΝ
ΠΡΕΙCΚΟΝΜΑΚΡΕΙΝΟΝΤΟΝ
ΑΓΝΟΝΚΑΙΔΙΚΑΙΟΝCΩΤΗΡΑ
ΜΑΝΝΟCΟΚΑΙΜΕΖΑΒΒΑΝ·
ΟΛΙΓΗΤΟΝΕΥΕΡΓΕΤΗΝ

ΙΟΥΛΙΟCΙΟΥΛΙ
ΚΑΙΤΕΤΕΙΜΗΜ
ΤΟΚΡΑΤΟΡΩ
CΕΙΛΗC · · ·
· · ΕΚΕΝ ΕΤΟΥ

ΤΟΜΝΗΜΙΟΝΤ
CΕΠΤΙΜΙΟCΟΔ
ΚΛΗΤΙΚΟCΑΙΡ
ΑΥΤΩΤΕΚΑΙΥ
ΤΕΛΕCΑΙΩΝΙ

XXV

RESORBISSVIETPROPAGATORESGENERISH
SIIMPIMPPETCONSTANTIVSETMAXIMI
SOSSIANOHIEROCLITEV.PRAES.PROVINC

From Mr. Dawkins's II. *plate of* GREEK *Inscriptions.*

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XXIV

·YAIONTONΠOΛ··THN
··CTHTHNHΠOΛIC

XXV

OCIOYΛIANOCEYCEBHCKAIΦIΛOΠATPIC
ETEIMHMENOCYΠOTΩNΘEIΩTATΩNA·
PATOPΩNTETAPTHC·TPATEIACEΠAPX
HC·· ·· ··

ΤΕΙΑCΕΠΑΡΧ
ΡΕΑCΤΕΙΜΗC
. . . . . .

ΕΝΕΞΙΔΙΩΝ
ΤΑΤΟCCΥΝ
ΥΤΟΥΝΑCΩΡΟΥ
ΝΟΙCΕΙCΤΟΠΑΝ

NVS
ELICITEPCONDIDERVNT

J. Mynde sculp.

NVM. I. *Taken from Montfaucon's* L'Antiq. Expli

ΑΓΛΙΒΩΛΩΚΑΙΜΑΛΑΧΒΗΛΩΠΑ
ΚΑΙΤΟΣΙΓ ΝΟΝΑΡΓΥΡΟΥΝΣΥΝΠΑΝΤΙΚ
Τ·ΛΥΡ·ΗΛΙΟΔΩΡΟΣΑΝΤΙΟΧΟΥΑΔΡΙΑΝΟΣΠΑΛΜΥΡ

ΣΩΤΗΡΙΑΣΑΥΤΟΥΚΑΙΤ·ΥΙΟΥΒΙΟΥΚΑΙΤΤΕΚΝΩΝΕΤΟΥ

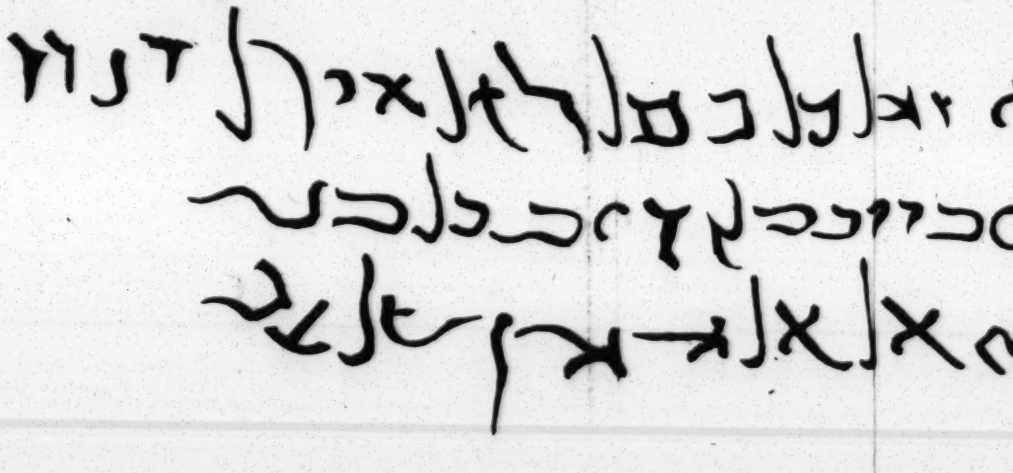
NVM. II. *Taken from Reland's* Palaeſt. Illuſt. Tom



From a Mſ. of the Rd. Mr. W. Halifax, who copied it

ΔΙΙ ΜΕΓΙΣΤΩ ΚΕΡΑΥΝΙΩ ΥΠΕΡ ΣΩΤΗΡΙΑΣ ΤΡ
ΣΕΒ ΤΟΥ ΚΥΡΙΟΥ ΑΓΑΘΑΝΓΕΛΟΣ ΑΒΙΛΗΝΟΣ ΤΗ
ΤΗΝ ΚΑΜΑΡΑΝ ωΚΟΔΟΜΗΣΕΝ ΚΑΙ ΤΗΝ ΚΛΙΝΗ
ΑΝΕΘΗΚΕΝ ΕΤΟΥΣ Ε̄Μ̄Ῡ ΜΗΝΟΣ ΛωΟΥ.....

NVM. III. *Taken from Reland's* Palaeſt. Illuſt. Tom.



From Spon's Miſcell. erudit. antiq. *pag.* 3.

SOLI SANCTISSIMO. SACRVM
TI. CLAVDIVS FELIX ET
CLAVDIA HELPIS ET
TI CLAVDIVS ALYPVS FIL. EORVM
VOTVM SOLVERVNT LIBENSMER
CALBIENSES DE COH. III.

tions publish'd by **Gruter, Spon, *and* Reland.**

iq. Expliq. *pl.* CLXXIX. Tom. II.

ΩΠΑΤΡΩΟΙCΘΕΟΙC
ΑΝΤΙΚΟΖΜΩΑΝΕΘΗΚΕ
ΛΜΥΡΗΝΟCΕΚΤΩΝΙΔΙΩΝΥΠΕΡ

ΝΕΤΟΥCΖΜΦΗΝΟCΠΕΡΙΠΙΟΥ
[illegible]

Illust. Tom. II. *pag.* 526. N. 1.

[illegible]

copied it from the Stone in 1691.

ΗΡΙΑC ΤΡΑ:ΑΔΡΙΑΝΟΥ
ΗΝΟC ΤΗC ΔΕΚΑΠΟΛΕΟC
ΤΗΝΚΛΙΝΗ..ΕΞΙΔΙΩΝ
ΩΟΥ.....

Illust. Tom. II. *pag.* 526. N. 2.

[illegible]

ntiq. *pag.* 3. N. 2.

Ô. SACRVM
LIX ET
S ET
FIL. FORVM
IBENS MERITO
OH. III.

J. Mynde sculp.

discovery of the Palmyrene alphabet. This, I told him, I could not then do; being engaged in a work of another nature, which I was obliged to dispatch with all the expedition possible. However, I assured him, that I would spend now-and-then an hour or two upon those inscriptions, when tired with working, and try what I could make of them. Having therefore borrowed a copy of the *Ruins of Palmyra*, on Saturday, January 12, 1754, I began, about five o'clock in the afternoon, to compare the 16th, 17th, and 19th Greek inscriptions with the 8th, 9th, and 10th Palmyrene, with all the attention I was capable of; and, in less than two hours time, did not only find out twenty letters of the Palmyrene alphabet, but likewise could interpret, to my own satisfaction, the three last-mentioned inscriptions. Between seven and eight o'clock, Mr. Godwyn sent me several words belonging to some of the other inscriptions, in the Hebrew character, the significations of which he had, as he apprehended, discovered, and the powers of eighteen different Palmyrene letters, as they appeared to him, in order to facilitate my inquiries. 'Tis worthy observation, that he assigned all these letters the same powers and places in the alphabet I had done; which confirmed me in my opinion, that I could not be very remote from truth. The form of the element *Koph* appeared to me at first to represent *Hheth*; but the word אקים AKIM, POSVIT, EREXIT, &c. soon afterwards gave me sufficiently to understand, that I was mistaken. We likewise both found, that this character sometimes occupied the place of *Mem*; though scarce the least affinity is to be observed between the forms of *Koph* and *Mem*, either in the Phœnician or the Chaldee alphabet.

The unexpected ſucceſs, I had met with, excited me afterwards to examine the letters and words of ſeveral other Palmyrene inſcriptions, even ſome of thoſe, which had no Greek ones to anſwer them. The latter however, for ſome time, not a little embarraſſed me; as I found the ſpirit and genius of them very different from thoſe of the others; though, with pleaſure, I obſerved, that in the moſt eſſential points the three completeſt of them agreed. But, notwithſtanding this, before the cloſe of Thurſday, January 17, 1754, I had diſcovered twenty-one letters of the Palmyrene alphabet; and could, as I imagined, tranſlate into Latin and Engliſh ſeven of the thirteen inſcriptions drawn in the Palmyrene character, three of which had no correſpondent Greek inſcriptions. In fine, before the end of February 1754, I thought myſelf able to explain all the thirteen inſcriptions, except the twelfth, which had ſeveral of its letters defaced; though I ſoon found out the date it bore, which indeed was viſible enough, as well as perceived the true turn and nature of it. I likewiſe had made myſelf fully acquainted with the forms and powers of all the Palmyrene letters, except thoſe of *Tzade*; which, if I am not greatly miſtaken, I have ſince certainly diſcovered. To the preceding ſhort account of the gradual progreſs made in the ſuppoſed interpretation of theſe inſcriptions, from the beginning to the end of it, I ſhall beg leave to ſubjoin here the Palmyrene alphabet, with ſome ligatures of the letters. After which will follow a Latin and Engliſh verſion of ſix Palmyrene inſcriptions, not a little illuſtrated by the Greek ones anſwering to them; and of four of thoſe, that have no correſpondent Greek inſcriptions. To which I ſhall add a few curſory remarks,

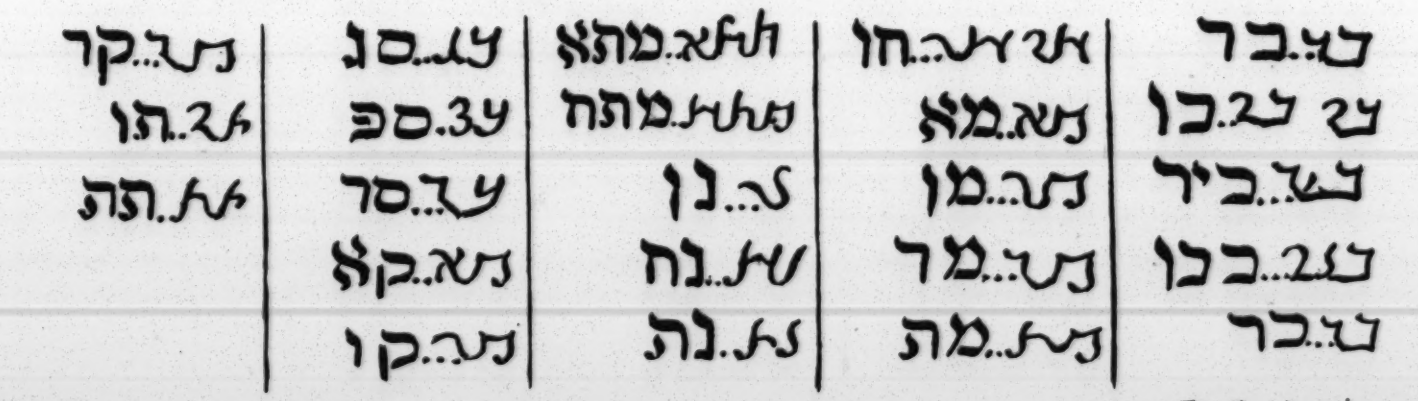
marks, and the particular days on which the explications of the inſcriptions were ſuppoſed to have been hit upon; that the true dates of theſe explications (if ſuch the learned gentlemen of the Royal Society ſhould allow them to be), may, with the utmoſt preciſion, be aſcertained.

pag. 693.

The PALMYRENE *Alphabet compared with the* HEBREW.

	Palmyr.	Hebr.		Palmyr.	Hebr.
Aleph	𐡀	א	*Lamed*	𐡋	ל
Beth	𐡁	ב	*Mem*	𐡌	מ ם
Gimel	𐡂	ג	*Nun*	𐡍	נ ן
Daleth	𐡃	ד	*Samech*	𐡎	ס
He	𐡄	ה	*Ajin*	𐡏	ע
Vau	𐡅	ו	*Pe*	𐡐	פ ף
Zain	𐡆	ז	*Tzade*	𐡑	צ ץ
Hheth	𐡇	ח	*Koph*	𐡒	ק
Teth	𐡈	ט	*Reſch*	𐡓	ר
Jod	𐡉	י	*Schin* / *Sin*	𐡔	ש
Caph	𐡊	כ ך	*Thau*	𐡕	ת

LIGATVRES *of the* PALMYRENE *Letters.*



Inſcription

Inſcription VIII.

See Plate XXV, *Number* VIII.

ספטמיוס ורוד קרטסטוס אפטרפא
דקנרא ואתבטא אקים יולים
אורלים ספטמיוס ידא הפקוס

SEPTIMIVM VORODEN EXCELLENTISSIMVM PROCVRATOREM DVCENARIVM ET ARGAPETAM POSVIT IVLIVS AVRELIVS SEPTIMIVS IADA HIPPICVS : : : : : : : : :

IVLIVS AVRELIVS SEPTIMIVS IADA HIPPICVS : : : : PLACED SEPTIMIVS VORODES, THE MOST EXCELLENT PROCVRATOR DVCENARIUS AND ARGAPETA, here.

January 12th, 1754.

Inſcription IX.

See Plate XXV, *Number* IX.

ספטמיוס ורוד קרטסטוס אפטרפא
דקנרא וארגבטא אקים יולים
אורלי : שלמא בר קסינא בר : : : : :

SEPTIMIVM VORODEN EXCELLENTISSIMVM PROCVRATOREM DVCENARIVM ET ARGAPETAM POSVIT IVLIVS AVRELIVS SALMA FILIVS CASSIANI FILII : : : : : : : :

IVLIVS AVRELIVS SALMA, THE SON OF CASSIANVS, THE SON : : : : : : : PLACED SEPTIMIVS VORODES, THE MOST EXCELLENT PROCVRATOR DVCENARIVS AND ARGAPETA, here.

January 12th, 1754.

Inſcription

Inſcription X.

See Plate XXV, *Number* X.

ספטמים וחר קרשסטס אפטרפא
דוקנרא די אקים ליקרא
יולים אולים נביבל בר שעחוחידא
אסטרגא די קלניא לחחמה
שנת דער בירה פללול

SEPTIMIVS VORODES EXCELLENTISSIMVS PRO-CVRATOR DVCENARIVS QVEM POSVIT AMICITIÆ NOMINE IVLIVS AVRELIVS NABIBAL FILIVS SAODOIDÆ (vel SAODVTI) DVCTOR EXERCITUS COLONIÆ HONORIS CAVSA ANNO DLXXIV MENSE APELLÆO.

SEPTIMIVS VORODES, THE MOST EXCELLENT PROCVRATOR DVCENARIVS, WHOM IVLIVS AVRELIVS NABIBAL, THE SON OF SAODVTVS, GENERAL OF THE COLONY'S FORCES, ON ACCOVNT OF FRIENDSHIP, PLACED here, IN ORDER TO DO HIM HONOVR, IN THE YEAR DLXXIV, and THE MONTH APELLÆVS.

January 12th, 1754.

Remarks on theſe Inſcriptions.

1. They ſeem to have appertained originally, tho' probably all of them were inſcribed either on pillars or pedeſtals, to three ſtatues of SEPTIMIVS VORODES, and to have applied to thoſe ſtatues the name of the perſon they repreſented. This is ſo clear from the ſtile of the inſcriptions themſelves, that I ſhall inſiſt no farther upon it here.

2. The

2. The two firſt of them are imperfect, the latter part of both having been apparently defaced.

3. The word NABIBAL, which is undoubtedly the true reading, as I have reſtored it here, that has been partly deformed and partly effaced by time, or elſe not exactly taken, in the third line of the tenth inſcription, may be recovered by the aſſiſtance of the Greek one anſwering to it.

4. I have taken the liberty to ſuppoſe the letter wanting in the beginning of the laſt word in the fourth line of that inſcription to be *Lamed*; which ſuppoſition the oriental critics will perhaps allow to be not very remote from truth. For, לרחמה, may either be expreſſed in Latin AMICITIÆ NOMINE, PRO AMICITIA, or (as *Lamed*, according to Schindler, *Lex. Pent.* p. 914, is ſometimes an article of the accuſative caſe) AMICVM SVVM; any of which expreſſions will come near enough to TON EAYTOY ΦIΛON, in the correſpondent Greek one, and be perfectly conſonant to the tenor of both inſcriptions.

5. The dialect, in which theſe Palmyrene inſcriptions are written, is moſt certainly the Syriac. This is rendered inconteſtable by the words אקים, SURGERE FECIT, POSUIT, EREXIT, STATUIT; בר, FILIUS; לי׳קרא, HONORIS CAUSA; לרחמה, or לרחמא, PRO AMICITIA; which manifeſtly belong to that dialect.

6. But, notwithſtanding this, ſeveral of the words they exhibit, ſuch as KPATICTOC, ΕΠΙΤΡΟΠΑ, ΑCTPATHΓA, DVCENARA, and COLONIA, are indiſputably of Greek and Latin extraction.

7. In the third line of the tenth inſcription, the word AVRELIVS wants the letter R; and in the fourth, the word ACTPATHΓA the letter T. That this is an undoubted

doubted truth, we may fairly infer from the third line of the eighth inſcription, and the ſecond of the fourth; where both theſe words drawn out at length, after the oriental manner, evidently occur.

8. The true reading of the word ΑΡΓΑΠΕΤΗΝ, in the ſixteenth and ſeventeenth Greek inſcriptions, which neither Dr. Halley, Dr. Bernard, Dr. Smith, nor Mr. Seller could aſcertain, is clearly diſcovered by the word (*a*) ARGABETA, in the ſecond line of the eighth, as well as of the ninth, Palmyrene inſcription; both of which are here exhibited to our view.

9. The tenth inſcription, the lateſt of thoſe whoſe dates have been preſerved, did not precede the 574th year of the æra of Seleucus, nearly coincident with the 263d of CHRIST; having been drawn out in the month *Apellæus*, or *December*, that year. This therefore will fix the date, that the correſpondent Greek inſcription originally bore; and conſequently render that inſcription ſomething more complete, than it was before.

10. It appears from theſe inſcriptions, that the letter *Vau* was ſometimes pronounced by the Palmyrenes, in the third century after CHRIST, like O, and at other times like OV or V; as alſo that they ſome-

(*a*) It ſeems to appear from the word ΑΡΓΑΒΙΑ, which in Leo denotes a ſort of *hamper*, uſed by the cavalry chiefly for the carriage of water, that ΑΡΓΑΠΕΤΗC, ΑΡΓΑΒΕΤΗC, or ARGABETA, ſignified an officer at Tadmor, whoſe province it was to ſupply the troops with water, in order to ſupport them in their march over the vaſt deſarts ſurrounding that place. Nor can it be doubted, but ſuch an officer muſt have been extremely neceſſary in a city diſtinguiſhed from all others by ſo remarkable a ſituation.

Vid. Leonis Imperat. *Tactic.* Cap. xiii. S. 11. Filip. Pigafetta, in annotat. ad Leon. ibid. Mauric. Lib. vii. p. 143. Upſaliæ, 1664. Vid. etiam Carol. Du Freſn. *Gloſſar. ad Scriptor. med. & infim. Græcitat.* in voc. ΑΡΓΑΒΙΑ.

times assigned to their *Beth* the power of the Greek *Pi*: of which other examples will likewise hereafter be produced.

Inſcription I.

See Plate XXIV, *Number* I.

מורא לבריך שמה
לעלמא יול אור.
: : ן לבונא בר אעילע.
זנביעא אקובעי
בירח טבת
ביומן ד
שנת דמד

NOMINI BENEDICTO TIMOR IN SECVLVM IVL. AVR. PRO PATRE NOSTRO (vel PRO PATRE) QVI ET FILIVS AILÆ (AILI vel ALLI) ZENOBII ACOBÆI (ACOBENSIS vel ACABENSIS) dedicavit MENSIS TEBETH DIE IV ANNO DXLIV.

TO THE BLESSED NAME BE FEAR FOR EVER: IVL. AVR. dedicated this ON OVR (or HIS) FATHER's ACCOVNT, WHO WAS THE SON OF AILA ZENOBIVS OF ACOBA, (or ACABA) THE FOVRTH DAY OF THE MONTH TEBETH, IN THE YEAR DXLIV.

January 15th, 1754.

Inſcription II.

See Plate XXIV, *Number* II.

לבריך שמה לעלמא טבא ורחמן.
מורא קר יול בר זברבול בר מילכום על
חיוהי וחיא אחוהי ביח תשרי
שנת דלג

NOMINI

NOMINI BENEDICTO IN SECVLVM BONO ET MISERICORDI TIMOR COR. IVL. FILIVS ZABDIBOLI FILII MILCOMI OB SVAM ET FRATRIS EIVS SALVTEM dedicavit MENSE TISRI ANNO DXXXIII.

TO THE BLESSED NAME, FOR EVER GOOD AND MERCIFVL, BE FEAR: COR. IVL. THE SON OF ZABDIBOL, THE SON OF MILCOM, dedicated this FOR HIS OWN HEALTH or SAFETY, AND THAT OF HIS BROTHER, IN THE MONTH TISRI, and THE YEAR DXXXIII.

January 17th, 1754.

Inſcription XIII.

See Plate XXV, *Number* XIII.

לבריך שמה לעלמא עבד שלמן בר נשא בר
חיצא סרכ על היוהי וחיא בנוהי
ה בירח ניסן שנת תמז : : : :

NOMINI BENEDICTO IN SECVLVM timor PARAVIT SALMON FILIVS NASÆ FILII HIZÆ (vel CHITZÆ) VTIQVE PROPTER SVAM ET LIBERORVM EIVS SALVTEM : : : : : : MENSE NISAN ANNO CCCCXLVII.

TO THE BLESSED NAME FOR EVER be fear: SALMON THE SON OF NASA, THE SON OF HIZA, (or CHITZA) PREPARED (or DEDICATED) this ON ACCOVNT OF HIS OWN HEALTH or SAFETY, AND THAT OF HIS CHILDREN, IN THE MONTH NISAN, and THE YEAR CCCCXLVII.

January 17th, 1754.

Remarks on theſe Inſcriptions.

1. The ſtones that exhibit them ſeem to have belonged to altars dedicated to *Jupiter*, the chief of

the gods, or rather to the Supreme Deity, either in conſequence of vows formerly made, on account of recoveries from dangerous fits of ſickneſs; or for the future health, ſafety, and preſervation of the perſon therein mentioned. This ſufficiently appears, not only from the Greek inſcription, which in ſome meaſure expreſſes the ſenſe of the firſt of them, but likewiſe from the tenor of them all. Nor can any thing be more clear and explicit, than the terms of which they are compoſed.

2. The aforeſaid Greek inſcription only informs us, that JVLIVS AVRELIVS, therein alſo mentioned, dedicated an altar, on his father's account, after he had prayed to the deity, for whom that altar was deſigned. But the Palmyrene one anſwering to it has preſerved the very form of this ſhort prayer, or ejaculation; which pretty nearly correſponds with that uſed by the prophet Daniel, in chap. ii. ver. 20. as well as a very noted one of the Rabbins. Nor is it altogether remote from a *(a)* ſcriptural expreſſion, that now and then occurs. Nay, that the antient Syrians, or rather the Syrian Jews, applied the word שמה, NAME, to GOD, or the Supreme Being, we learn from ſome (*b*) good authors. This opens a large field for reflections, and ſuggeſts to us ſeveral remarkable particulars relating to the religion, or rather religions, that prevailed at Tadmor, about the middle of the third century.

3. Thoſe particulars are likewiſe very clearly pointed out to us by the ſecond Palmyrene inſcription, which contains a ſort of ſhort prayer, or eja-

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(*a*) Val. Schind. *Lex. Pentaglot.* p. 247—250, 1886, &c.
(*b*) Edm. Caſtel. *Lex. Hept.* p. 3772.

ulation,

culation, not unlike one in the beginning of the *(a) Liturgy of the Syrian Jacobite Chriſtians*, a Latin verſion of which has been publiſhed by M. Renaudot. The thirteenth Palmyrene inſcription gives us to underſtand, that the ſame religion, or religions, had been introduced at Tadmor, ſoon after the beginning of the ſecond century.

4. As a ſimilar *(b)* inſcription ſtill remains, or was very lately to be ſeen, near the well, or current of hot ſulphureous waters, which undoubtedly conſtituted in antient times one of the hot ſulphureous baths of Tadmor, it ſeems not improbable, that the waters of thoſe baths were medicinal; and that by bathing themſelves therein, or drinking them, the Palmyrenes were frequently cured of various diſtempers and indiſpoſitions. This, I ſay, appears at leaſt not improbable, from the matter and ſituation of that inſcription, in conjunction with thoſe which I have here endeavoured to explain, and which had in all likelihood at firſt either the ſame or a ſimilar ſituation. So that theſe inſcriptions, every thing conſidered, may be deemed mutually to ſupport and illuſtrate one another.

5. From the word LABVNA, לבונא; P RO PATRE NOSTRO, in the third line of th^e firſt inſcription, we may infer, that the ſecond letter *Aleph* was either inadvertently omitted by the inſcriber, or virtually contained in the firſt letter *Lamed*, or *Lomad*. For LABVNA muſt be rendered into Latin PRO PATRE NOSTRO, or ſimply PRO PA-

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a) Euſeb. Renaud. *Liturgiar. Oriental. Collect.* Tom. ii. p. 2. Pariſiis, 1716.

(b) Edw. Bernard. *Monument. Palmyren.* p. 4. *Philoſoph. Tranſact.* Vol. xix, N. 217, p. 109.

TRE;

TRE; the Syrians *(a)* and Arabs ſometimes applying it in that ſenſe as a title, or mark of diſtinction, to the Jacobite patriarchs, and probably others, as we learn from Al Makîn. Nay, it ſeems as natural to ſuppoſe, that the Syrians uſed the word ABVNA promiſcuouſly for OVR FATHER, and FATHER; as that ABA, or ABBA, ſhould have been admitted by them in two ſimilar ſignifications. Nor can the words ANTI ΠΑΤΡΟΣ, in the correſpondent Greek inſcription, poſſibly anſwer to any other word, in that I am conſidering, than LABVNA. This will ſuggeſt to us one or two curious obſervations, which I have not time at preſent ſo much as to touch upon.

6. The Syriac זנבישא, ZENOBIA, is expreſſed by the Greek ZHNOBIOY; and conſequently muſt be looked upon as a maſculine proper name. But whether the ſame letters, amongſt the Palmyrenes, formed the proper name of *Zenobia*, the famous queen or empreſs of that nation, I cannot yet take upon me to determine.

7. ACOBA, or ACABA, the city, as it ſhould ſeem, to which ZENOBIVS, mentioned in the firſt inſcription, did originally belong, might poſſibly have been the capital of *Acabene*, a province or diſtrict of Meſopotamia, a region ſeparated from Palmyrene by the Euphrates. This diſtrict is placed by Ptolemy at no great diſtance from the Tigris; and its capital might have been called by the Palmyrene Greeks, in the third century, AKOΠA. But that this was really

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(a) Val. Schind. *Lex. Pentaglot.* p. 8. Geor. Elmacin. *Hiſt. Saracen.* p. 144, 145, 151, 152, & alib. paſs.

the

the town the inſcriber had in view, I muſt not preſume poſitively to affirm.

8. From the firſt of theſe Palmyrene inſcriptions, if the ſixth line of it be exactly copied, and the Greek one anſwering to it, we may fairly collect, that the fourth day of the month *Tebeth* was coincident with the twenty-fourth of *Aydynæus*, the Macedonian month; and conſequently that thoſe two months were not preciſely the ſame, as we find intimated *(a)* by Dr. Fabricius. The former of them began on the twenty-firſt day of the latter, if the authority of our inſcription in this point may be intirely depended upon. Hence likewiſe it moſt evidently appears, that *Tebeth* was the name of a Syrian, as well as of an Hebrew, month.

9. Though the Palmyrene dialect be almoſt, in all reſpects, the ſame with the Syriac; yet it muſt be allowed, that ſome Hebrew words occur in theſe inſcriptions. Of this מורא, in the firſt and ſecond, and רב, in the fifth, to omit other inſtances of the ſame kind, are inconteſtable proofs. For which we may eaſily account, if we conſider, that many *(b)* Jews, as well as Chriſtians, were ſettled in the territories of the Palmyrenes. 'Tis remarkable, that though the word בר, BAR, is uſed here for SON in the enumerations of deſcents; yet בנוהי, according to the Syriac form, HIS CHILDREN, preſents itſelf to our view towards the cloſe of the thirteenth inſcription. As to the word חיורי, it muſt be owned to be likewiſe of

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(a) Jo. Albert. Fabric. *Menolog.* p. 16, 42. Hamburgi, 1712.
(b) Seller's *Antiq. of Palmyr.* Chap. xix.

the

the Syriac form; and conſequently not to have been improperly uſed by the Palmyrenes. The ſame may be ſaid of the word אחוהי, apparently deducible from (*a*) אחא, FRATER; as the letters ח and ה are of the ſame organ, have frequently a pronunciation extremely ſimilar, and were therefore undoubtedly often taken and uſed for one another. Unleſs we ſuppoſe, that the word was originally upon the ſtone אחוהי, which will bring it to the pure Syriac form; and this for various reaſons, eſpecially as that very term is exhibited by the third inſcription, the moſt antient of all, in the very ſame ſenſe, I am inclined to believe.

10. That the proper names ZABDIBOL, MILCOM, SALMON, NASA, and HIZA or CHITZA, in the ſecond and thirteenth inſcriptions, which have no Greek ones to anſwer them, are either of Syriac, Chaldee, or Hebrew extraction, from ſome of the (*b*) beſt oriental lexicographers moſt clearly appears. As for HIZA, or CHITZA, this muſt be allowed to be a pure Syriac word; though I remember not to have met with it as a proper name in any antient author. The ſecond inſcription bears date in the month *Tiſri*, and the 533d year of the æra of Seleucus, or A. D. 222, ſoon after Alexander Severus had aſcended the imperial throne. The thirteenth is 76 years older, having been drawn A. D. 136, in the reign of the emperor Hadrian, and about two years before the death of that prince. The altar on which the firſt appears inſcribed was

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(*a*) Val. Schind. *Lex. Pent.* p. 54. *Verſ. Syr.* in GEN. iv. 8, 21.
(*b*) Val. Schind. *Lex. Pentaglot.* paſs. Edm. Caſtel. *Lex. Heptaglot.* paſs.

erected

erected in the year of CHRIST 233, in the month *Tebeth*, or *Aydynæus*, when Alexander Severus was engaged in the Parthian war. 'Tis obſervable, that the forms of ſome of the letters, and particularly that of the *Jod*, or *Jud*, in the third and thirteenth inſcriptions, conſiderably differ from thoſe of the ſame elements, that have been preſerved by the others, which are of a later date. But notwithſtanding this, that all the letters forming theſe Palmyrene inſcriptions are to be deduced from the ſame ſource, will, I believe, be univerſally allowed.

Inſcription XII.

See Plate XXV, *Number* XII.

אמתא בר תבלעקב בר נשא
אמתאלהסל בר והבלת

AMTACHA FILIVS TEBELACABI FILII NASÆ AMTALEHSAL FILIVS VAHBALATHI.

AMTACHA THE SON OF TEBELACAB, THE SON OF NASA; AMTALEHSAL THE SON OF VAHBALATH.

January 17th, 1754.

Inſcription XI.

See Plate XXV, *Number* XI.

בלמיחל בר מוגיאתת
מעזו בר והבלת

BALMICHAL FILIVS MOGIATHATHI MAZO FILIVS VAHBALATHI.

BALMICHAL THE SON OF MOGIATHATH, MAZO THE SON OF VAHBALATH.

January 17th, 1754.

Remarks on theſe Inſcriptions.

1. They ſeem both to have been mutilated, tho' the eleventh has ſuffered more than the other; ſome of its letters being partly defaced, and partly deformed, by time.

2. That they conſiſt of the proper names of men, connected by the word BAR, SON, is obvious to every one, who reads them.

3. That theſe men were perſons of diſtinction, and filled ſome of the higheſt poſts in the ſtate, there is great reaſon to believe.

4. That the names themſelves, as exhibited here, are of oriental extraction, tho' the reaſon of their impoſition, at this diſtance of time, is not known, (*a*) can by no means be denied.

5. In the twelfth inſcription, as well as the eleventh, tho' in the latter it is ſomething deformed, the proper name VAHBALATH, or VABALATHVS, (which has alſo been preſerved by coins) in the Palmyrene character, manifeſtly occurs.

6. That the dates of theſe inſcriptions, if any were ever viſible in them, muſt have been poſterior to the 447th year of Seleucus, or the 136th of CHRIST, the forms of the letters themſelves, and particularly that of the *Jod*, or *Jud*, evidently prove.

(*a*) Val. Schind. *Lex. Pentaglot.* p. 1048, 1220, 1050, 1959, 1960, 1372, &c.

Incription V.

See Plate XXIV, *Number* V.

חלקא דנה די ספטמיוס אירן בר
אדינת סנקלטיקא נהירא ורש
תדמור די אקים לה אורליס
פלינוס בר מריא פלינא ודי פלחא
דב לגיונא די בתרא ליקרא ביתה
תשרי די שנת דסג

SORS ſive PORTIO eſt HÆC SEPTIMII ÆRANÆ FILII ODÆNATHI SENATORIS CLARISSIMI ET VIRI PRIMARII civitatis TADMOR QVAM EI EREXIT AVRELIVS PHILINVS FILIVS MARII PHILINI MILES EMERITVS IACVLATOR ſive PILANVS LEGIONIS PARTHICÆ HONORIS CAVSA MENSE TISRI ANNI DLXIII.

THIS IS THE LOT, or PORTION, OF SEPTIMIVS ÆRANES, A MOST ILLVSTRIOVS SENATOR, AND A PRINCIPAL PERSON IN THE CITY OF TADMOR; WHICH AVRELIVS PHILINVS, THE SON OF MARIVS PHILINVS, A VETERAN OF THE PARTHIAN LEGION, WHO HAD SERVED AS A LANCIER, ERECTED TO HIS HONOVR, IN THE MONTH TISRI OF THE YEAR DLXIII.

February 2d, 1754.

Infcription VII.

See Plate XXIV, *Number* VII.

חלקא דנה די יולים אורליס
שלמלת בר מלא עברי רב שירתא
די אקימת לה בולא ודמס ליקרא
די אסם שירתא מער מגן יסה
שנת דסט

SORS ſive PORTIO eſt HÆC IVLII AVRELII SALMALATHI FILII MALÆ HEBRÆI PRINCIPIS vel DVCTORIS CATERVÆ Mercatorum QVAM STATVIT EI SENATVS POPVLVSQVE QVOD DOMVM REDVXIT CATERVAM Mercatorum DE RE FAMILIARI GRATIS eam SVSTENTANS ANNO DLXIX.

THIS IS THE LOT, or PORTION, OF IVLIVS AVRELIVS SALMALATH, THE SON OF MALA, A IEW, PRINCE, CHIEF, or LEADER, OF THE CARAVAN of Merchants; WHICH THE SENATE AND PEOPLE HAVE DECREED HIM, BECAVSE HE CONDVCTED HOME THE CARAVAN, AND SVPPORTED IT AT HIS OWN EXPENCE, IN THE YEAR DLXIX.

February 9th, 1754.

Remarks on theſe Inſcriptions.

1. From the words אדינת, אירן, AIRAN, ODINATH, or ÆRAN, ODÆNATH, and בולא, together with the words AIPANHN OΔAINAΘOϒ, H BOϒΛH, in the correſpondent Greek inſcription, it plainly appears, that the letter *Aleph*, amongſt the Palmyrenes, ſometimes anſwered to A, ſometimes to E, and ſometimes to O, about the middle of the third century.

2. From the words סנקלט׳קא, SENATORIS, מריא פלינא, MARII PHILINI, אורלים פלינוס, AVRELIVS PHILINVS, &c. we may collect, that one of the Palmyrene terminations of Greek and Latin words in the oblique caſes was א; but that the people of Tadmor, in the nominative caſe, when they wrote ſuch words in their own character, generally preſerved the Greek and Latin terminations.

tions. See the eighth, ninth, and tenth of the Palmyrene inſcriptions.

3. The proper name מלא, MALA, in the ſeventh inſcription, ſeems to anſwer to MAΛHC, in the ſeventh of Dr. Bernard's Greek Palmyrene inſcriptions; though the terminations of thoſe words are not intirely the ſame. The proper names, or ſurnames, שלמא ידא are alſo in like manner repreſented by CAΛMHC IAΔHC, as has been already obſerved. As for the name, or ſurname, SALCALATH, or SALMALATH, it only occurs in the ſeventh of Mr. Dawkins's Palmyrene inſcriptions, which I have here been attempting to explain.

4. Though the dialect, in which the two inſcriptions I am conſidering are written, be apparently the Syriac; yet the word רב, IACVLATOR, in one of them, as has been already remarked, is undoubtedly Hebrew. As for בולא, BOYΛH, דמס, ΔHMOC, and סנקלטיקא, CYNKΛHTIKA, or rather CYNKΛHTIKON, they are moſt evidently Greek. In one of theſe inſcriptions, the word רש, PRINCEPS, VIR PRIMARIVS, appears without either the א or the י, inſerted as the middle letter; which ſometimes, tho' very rarely, happens. The word שירתא, CATERVA, is applied to the Iſhmaelites, as merchants, *Gen.* chap. xxxvii, ver. 25. in the Syriac verſion; which, with the word APXEMΠOPON, exhibited by Mr. Dawkins's thirteenth Greek inſcription, agreeing in ſignification with our רב שיחדא, plainly ſhews, that I have hit upon the true ſenſe of that term here. The ſurname PHILINVS and the name MARIVS are only viſible in the fifth of theſe Palmyrene inſcriptions;

Mr.

Mr. Dawkins's eleventh Greek one preſenting nothing of that kind to our view, but ΑΥΡΗΛΙ : : : : and part of the word ΗΛΙΟΔΟΡΟΥ, if that be exactly taken. We may conclude, from the fifth Palmyrene inſcription, that the people of Tadmor, if not the Syrians in general, when it was written, called *Parthia* BATRA, or BATRIA; which in ſound approaches pretty near to the BACTRIA of the antients. The name תרמור, TADMOR, conſiſting of five letters, and not of four only, as has hitherto been generally, if not always, ſuppoſed, occurs in the ſame inſcription. The two laſt words of the fourth line of the ſeventh have either not been ſo well preſerved as the others, or not ſo accurately taken. They may nevertheleſs be read either מן כיסה, EX SVO MARSVPIO, or מגן יסה, GRATIS DE RE FAMILIARI; either of which lections is conſonant enough to the tenor of the inſcription. The latter, which I have choſen here, ſeems however to be better ſupported by the correſpondent Greek inſcription; as the Syriac מגן exactly anſwers to the adverb ΠΡΟΙΚΑ there. Should the critics allow the words מגן יסה, as they now appear upon the face of the inſcription, to ſtand; the Syriac יסה muſt be of the ſame import with the Hebrew ישה, PECVNIA, DIVITIÆ, RES FAMILIARIS, &c. though it has for its middle letter *Samech*, inſtead of *Schin*. Nor indeed is this to be wondered at, as the Syrians ſometimes uſed the former for the latter of thoſe elements. That word, upon the foregoing ſuppoſition, ſuffers here an ellipſis of the particle ב BE; as is evident from Mr. Dawkins's thirteenth Greek inſcription, with the fragments of which the ſeventh Palmyrene one extremely

extremely well agrees. Such ellipſes as this were antiently not uncommon, according to Noldius *(a)*.

5. From the ſeventh inſcription it appears not improbable, that ſome eminent Jewiſh merchants, about the middle of the third century, reſided at Tadmor. That inſcription therefore may perhaps be allowed to ſupport the authority of Photius; who has not ſcrupled to aſſert, that the famous Zenobia herſelf profeſſed the Jewiſh religion (*b*).

6. From the two inſcriptions now before us, and the others written in the ſame character, we may fairly infer, that the uſe of the Chaldee letters (between which and the Palmyrene there is ſo ſurprizing an affinity, that they may not improperly be conſidered as the ſame) prevailed at Tadmor, and in all the neighbouring parts of Syria, that were at no great diſtance from the confines of Chaldæa, or Irâk, in the firſt, ſecond, and third centuries after CHRIST. For the oldeſt of them was drawn out in the month *Elul*, or *September*, and the year of Seleucus 360, nearly coincident with the 49th of CHRIST; the thirteenth of them in the month *Niſan*, or *April*, of the year of Seleucus 447, anſwering to the year of CHRIST 136; the fifth, that I have been juſt attempting to explain, in the month *Tiſri*, and the year of Seleucus 563, or of CHRIST 252, when Trebonianus Gallus directed the Roman affairs; the ſeventh, that I have been likewiſe now endeavour-

(*a*) Chriſtian. Nold. *Concordant. Particular. Ebræo-Chaldaic.* &c. p. 164, &c. Jenæ, 1734.

(*b*) Phot. *Cod.* σξ', p. 1469, 1470. Rothomagi, 1653.

ing

ing to decipher, in the year of Seleucus 569, which correſponds with the 258th of the Chriſtian æra; and all the others, that bear any dates, in the ſame century. Neither the preſent Syrian letters therefore, nor the Mendæan or Nabathæan characters, ſeem to have been uſed by the Syrians bordering upon Irâk, during the three firſt centuries after CHRIST; nor conſequently in any other interval whatſoever preceding the commencement of that period. So that theſe inſcriptions may, with ſufficient propriety, be conſidered in the light of manuſcripts, written in the Chaldee or Hebrew character, 1500, 1600, and even 1700 years old.

7. With regard to the numeral characters, exhibited both by the inſcriptions at preſent in view, and the others tranſmitted down to us in the Palmyrene language, it may not be improper to obſerve, that the letter *Ajin*, or at leaſt a character ſimilar to it, in the firſt place, annexed to a ſort of mark or ſign of an unuſual form, ſometimes denoting TEN, ſtands for 500, and in the laſt place alone for the number FIVE. The Palmyrene *Pe*, which reſembles the figure 3, repreſents the number TWENTY; and unity is expreſſed by the Pelaſgic or Attic character I, which was likewiſe antiently uſed by the Romans. Hence it appears, that this kind of notation was undoubtedly very antient in the Eaſt; and might poſſibly have prevailed amongſt the Syrians, ſeated not far from the frontiers of Arabia and Irâk, and even amongſt the Arabs and Chaldæans themſelves, ſeveral centuries before the birth of CHRIST; though from whence the Syrians firſt received it, or how they came firſt

to

to hit upon it themſelves, I have not yet been able to diſcover.

To the preceding remarks many others might have been added, relating to the hiſtory of Tadmor, as well as to the genius, government, cuſtoms, religion, language, &c. of the inhabitants of that once moſt flouriſhing city. But, as I intend hereafter, when I can find a little relaxation from the buſineſs, in which I am at preſent engaged, if it ſhall pleaſe GOD to grant me health, to publiſh a diſſertation upon the inſcriptions tranſmitted down to us in the Palmyrene character, and to conſider every thing material deducible from them in a proper manner; I have here only attempted a bare tranſlation of the major part of them, and laid down a few curſory remarks, which upon my firſt peruſal of them occurred, in order a little to ſupport and illuſtrate that tranſlation. Beſides, a minute diſcuſſion of all the abovementioned particulars would have ſwelled this paper greatly beyond the bounds of a letter; whereas my preſent intention is, only to ſubmit a few memoirs relating to theſe celebrated monuments of antiquity to the judgment of our moſt learned and illuſtrious Society; that by this means my deſign may in ſome meaſure tranſpire, and come to the knowlege of at leaſt the moſt diſcerning part of the public. I ſhall offer no apology for the trouble now given you, Sir, as I was informed that a ſight of theſe memoirs would not be unacceptable to you. In the mean time, you will permit me only to add, that I muſt acknowlege myſelf obliged to the Reverend and very Learned Mr. Sanford, Fellow of Balliol College, as well as to Mr. Godwyn, for many

valuable hints, from time to time communicated to me; and that I am, with the moſt perfect eſteem,

SIR,

Your moſt obedient humble ſervant,

John Swinton.

LETTER II.

Reverend Sir, Chriſt-Church, Oxford, June 27, 1754.

Read July 5, 1754. THE favourable reception, which my laſt letter met with from the Royal Society, has encouraged me to trouble you with another upon the ſame ſubject; and to ſend you a Latin and Engliſh verſion of Mr. Dawkins's three remaining Palmyrene inſcriptions, attended by ſuch ſhort remarks, as were drawn up in order a little to illuſtrate and explain the former. And this I have been the more readily induced to do, as I have, I think, ſince diſcovered more fully the true notation of the Palmyrenes, at leaſt from UNITY to a THOUSAND, and am thereby enabled to correct one or two ſlight errors, which had before eſcaped me. I ſhall therefore, without any farther preface or introduction, immediately proceed to the point in view, and conſider the three inſcriptions now before me, in the ſame manner I did the preceding ones; eſpecially, as I have had the pleaſure to find, that the method by me formerly obſerved did not prove unacceptable to ſo conſiderable a part of the learned.

Inſcription

Inſcription IV.

See Plate XXIV, *Number* IV.

חלק יולים אוליס זבדלא בר מלכו בר מלכו כר
נשום די הו אסטרטג לקלניא במיתויתא די
אלהא אלכסנדחס קסר ושמש כדיא ואתן ר
קריספינוס היגמונא וסריאן לפאין לדיניא
זבד בסגיאלוה וארב שום וחס נדיאין שגיאין שגיאין
והבר עם ראש כנתנת מטל כות סהד לה ירחבעל
אלהא ואפ יולים די סוא ורמים מרתא
די אקימת לה בולא ודמוס ליקרא שנת דנר

SORS ſive PORTIO IVLII AVRELII ZABDILÆ FILII MALCHI FILII MALCHI FILII NASSVMI QVI IPSE DVCTOR EXERCITVS COLONIÆ IN ADVENTV DEI ALEXANDRI CÆSARIS ET MINISTER IVXTA SVFFICIENTIAM ET PERENNITATEM (i. e. DIGNVS ET PERPETVVS LEGATVS) RVTILII CRISPINI PRÆFECTI ET CHILIADVM (COPIARVM vel VEXILLATIONVM) AMANDATARVM (vel DEDVCTARVM) IN IVDICEM (i. e. IVDEX ſive QVÆSTOR) ET DE PECVLIO SVO (ſeu RE FAMILIARI) iis DONAVIT ET REPOSITO frumento PROSPEXIT ET PLVRIMA TRIBVTA CVRAVIT ET PRÆCLARE REM EGIT SECVNDVM DONA (ſive PLENA MANV) PROVT hæc TESTATVS EST EI DEVS IARIBOLVS ET IVLIVS (PHILIPPVS) ETIAM (vel PROPTER HÆC A DEO IARIBOLO ET IVLIO (PHILIPPO) ITIDEM CONFIRMATA atque COMPROBATA) QVI DIGNVS ET EXCELSVS eſt DOMINVS QVAM EI POSVIT SENATUS POPVLVSQVE HONORIS CAVSA ANNO DLIV.

THE LOT, or PORTION, OF IVLIVS AVRELIVS ZABDILA, THE SON OF MALCHVS, THE SON OF MALCHVS, THE SON OF NASSVM, WHO WAS COMMANDER IN CHIEF OF THE COLONY'S FORCES, WHEN THE GOD ALEXANDER CÆSAR ARRIVED HERE, AND THE WORTHY AND PERPETVAL LIEVTENANT OF RVTILIVS CRISPINVS THE PRÆFECT, AND JVDGE, or QVÆSTOR, OF THE AVXILIARY TROOPS, WHOM HE SVPPORTED AT HIS OWN EXPENCE, AND WAS THEIR PVRVEYOR, AND RECEIVED THE TRIBVTE, AND PRESIDED WITH GREAT PRVDENCE AND GENEROSITY; FOR THESE SERVICES, AS ATTESTED BY THE GOD IARIBOLVS AND THE EXCELLENT LORD IVLIVS (PHILIPPVS), THE SENATE AND PEOPLE HAVE ERECTED THIS TO HIS HONOVR, IN THE YEAR DLIV.

February 16th, 1754.

Inſcription VI.

See Plate XXIV, *Number* VI.

בולא ודמוס ליולים אורלים
בר אדינת מרא סלוקוס בר
ושאילאדי שמש ושפר : : : : :
להון באסטרטגא הז מגר לבולא
נדבו ליקרא בירח תשרי שנת רסו : : : :

SENATVS POPVLVSQVE IVLIO AVRELIO FILIO ODÆNATHI MADDÆ (vel MATTHÆ) SELEVCO FILIO : : : : : ET SEEILADIO IIS SERVIENTI ſeu MINISTRANTI et BENE SE GERENTI NEC NON DVCTORI EXERCITVS IPSI A SENATV : : : : : : : HONESTATO (vel CVM IPSO EXIMIO EXERCITVS DVCTORE SENATVS POPVLIQVE PALMYRENI) SPONTE DONAVIT (i. e. DECREVIT) HONORIS CAVSA MENSE TISRI ANNI DLXVI.

THE

THE SENATE AND PEOPLE HAVE VOLVNTARILY GIVEN, or DECREED, these TO IVLIVS AVRELIVS, THE SON OF ODÆNATHVS; MADDA, or MATTHA, SELEVCVS, THE SON OF ::::::; AND SEEILADIVS, WHO SERVED THEM WELL; AS ALSO TO THE GENERAL, HONOVRED BY THE SENATE :::::::: (or TOGETHER WITH THE RENOWNED GENERAL OF THE SENATE AND PEOPLE OF TADMOR) IN ORDER TO DO THEM HONOVR, IN THE MONTH TISRI OF THE YEAR DLXVI.

February 21st, 1754.

Inſcription III.

See Plate XXIV, *Number* III.

ירח אלול שנת שס
אנח מנא רנה ועלתא די
בדו ומרבול שמש וזביד
בנו מלכו בר יריעבל בר נשא
סימת מדא כר עברבל די מ::::
פחד בני מגר תל שמש::::::
יסהבית אבוה נעל::::::
חויהון וחוי אחוה
ובניהון

MENSE ELVL ANNI CCCLX DONVM HOC ATQVE ARA ::: BADI AMRISAMSÆ ET ZEBIDÆ FILIORVM MALCHI FILII IARIBOLIS FILII NASÆ REPOSITA ſive DONARIA MADDÆ ſeu MATTHÆ FILII ABDEBALIS QVOD CVM ÆDIFICIVM COLLAPSVRVM TIMERET SERVVS EIVS ASCENDENS PATREM SVVM SERA vel PESSVLO CLAVSVM ABSTVLIT ET PROPTER SALVTEM EORVM ET SALVTEM FRATRIS EIVS ET LIBERORVM SVORVM.

IN

IN THE MONTH ELVL OF THE YEAR CCCLX, THIS GIFT AND ALTAR WERE PLACED here BY ::::: BADVS, AMRIO'L SHEMS, AND ZEBIDA, THE SONS OF MALCHVS, THE SON OF IARIBOL, THE SON OF NASA: RICH PRESENTS, or OFFERINGS, were likewiſe placed on or near the Altar BY MADDA, or MATTHA, THE SON OF ABDEBAL, BECAVSE THAT, WHEN HE WAS AFRAID OF A HOVSE FALLING, A SERVANT OF HIS WENT * VP, AND CARRIED OFF HIS FATHER, WHO WAS LOCKED, or BOLTED, IN; AND FOR THEIR SAFETY, HEALTH, or PRESERVATION, AS WELL AS FOR THE SAFETY, HEALTH, or PRESERVATION, OF HIS BROTHER, AND THEIR CHILDREN.

February 25th, 1754.

Remarks on theſe Inſcriptions.

1. It may be proper to obſerve here, that I have adhered as cloſely to the original Palmyrene, in my Latin verſion of theſe three laſt inſcriptions, which are much more abſtruſe than the others, as the genius of the Latin language would permit; though I have taken greater liberties in the Engliſh tranſlation, in order to render them more intelligible to the generality of our Engliſh readers.

2. The fourth inſcription has been in the main extremely well preſerved, and diſcovers more of the true turn and genius of the Palmyrene dialect, than any of the others. Of this במיתותא, IN ADVENTV, though it varies from the pure Syriac form; כריא ואתן, IVXTA SVFFICIENTIAM ET PERENNITATEM, an idiom that ſufficiently points out to us the nature of the language ſpoken

* See Dr. Shaw's *Travels*, p. 276.

by

by the Palmyrenes; לפאי, CHILIADES, for ſo it ought to be read, a word that ſeems peculiar to that language, though apparently of Hebrew or Syriac extraction; בסגיאלוה, DE PECVLIO; נריאין שגיאין שגיאין, TRIBVTA MVLTA MVLTA, i. e. TRIBVTA PLVRIMA; כנתנת, SECVNDVM DONA, i. e. PLENA MANV; מרתא, DOMINVS; to omit other inſtances that might be produced, are indubitable proofs. The words IVLIVS, AVRELIVS, COLONIA, CÆSAR, CRISPINVS, ΗΓΕΜΟΝΑ, ΔΗΜΟC, ΒΟΥΛΗ, and ΑΛΕΞΑΝΔΡΟC diſcover at firſt ſight the ſources from whence they are to be deduced. The laſt of thoſe words has been a little injured by time, ſeveral of the forms of its letters being conſiderably altered. The third and fourth elements of the fifth line of this inſcription, as well as the ſecond, third, twenty-ſecond, and twenty-eighth (*Thau* and *Nun* in the Palmyrene alphabet being extremely ſimilar) of the fourth, have likewiſe met with the ſame fate; to all which I have endeavoured to reſtore their primitive powers and forms. In fine, this inſcription may be conſidered as a tolerable good ſpecimen of the language or dialect of the Palmyrenes (*a*).

3. The words בר מלכו בר מלכו, BAR MALCHV BAR MALCHV, in the beginning of this inſcription, evidently confirm the ingenious conjecture of the learned Mr. William Baxter, mentioned by Dr. Halley; according to which, ΔΙΣΜΑΛΧΟΥ, in the correſpondent Greek inſcription,

(*a*) Vid. Schind. *Lex. Pentaglot.* & Caſtel. *Lex. Heptaglot.* paſs.

was

was inſerted there inſtead of MAAXOY TOY MAAXOY. They at the ſame time moſt clearly evince, notwithſtanding what has been advanced to the contrary by Dr. Bernard and Dr. Smith, that the word ΔΙΣΜΑΛΧΟΥ is not of oriental extraction.

4. The ſurname PHILIPPVS, which ought to have followed IVLIVS, in the ſeventh line of this inſcription, does not appear; though a chaſm, or vacant ſpace, capable of containing a word of that ſize, preſents itſelf there to our view. This, in conjunction with the parallel chaſm, exhibited by the Greek inſcription, expreſſing much of the ſenſe of the Palmyrene one I am conſidering, amounts, in my opinion, to an almoſt irrefragable proof, that this ſurname, as our great Dr. Halley formerly ſuppoſed, was, after *Philip*'s treaſon to the emperor Gordian came to light, purpoſely effaced.

5. The laſt letter but one of the ſixth line of our Palmyrene inſcription ſeems to reſemble *Daleth* or *Reſch*, whereas it was undoubtedly deſigned at firſt for *Ajin*; ſo that the name to which it belongs ought to be written in Hebrew or Chaldee letters ירחבעל, IAREHBAAL, IARIHBAL, or IARIBOL, i. e. DOMINVS LVNVS. From whence we may certainly infer, as Dr. Halley has obſerved, that this deity was the DEVS LVNVS worſhiped by the antient Syrians. I have therefore taken the liberty to reſtore the true reading here, by converting the *Daleth* or *Reſch* into *Ajin*; as alſo to inſert the word בר. BAR, FILIVS, notwithſtanding it has probably for many ages diſappeared, as the laſt in the firſt line of the inſcription.

6. The numeral characters preſerved at the end of this inſcription merit the particular attention of the

the learned. From them, in conjunction with the numeral letters ſtill viſible in the Greek one anſwering to it, we may certainly conclude, that the unuſual mark, formerly mentioned, alone properly denotes TEN, and with unity prefixed an HUNDRED. This obſervation will enable me to correct one or two ſmall errors, in the dates before aſſigned the ſecond and tenth Palmyrene inſcriptions. The former was drawn in the year of Seleucus 533, not 523, as I then imagined; and the latter in 574, not 564, as I ventured to aſſert. In fine, by the ſole aſſiſtance of theſe characters, and thoſe of the ſixth and ſeventh Palmyrene inſcriptions, as explained by the correſpondent Greek numeral letters, I have conſtructed the following table of numbers, from UNITY to a THOUSAND, according to the true and genuine notation of the Palmyrenes.

PALMYRENE *Numerals from* One *to a* Thouſand. *pag. 721.*

CLX 333⊃1	C ⊃1	XXXI 1⊃33	XVI 1Y⊃	I 1
CIXX ⊃333⊃1	CI 1⊃1	XXXII 11⊃33	XVII 11Y⊃	II 11
CLXXX 3333⊃1	CII 11⊃1	XXXIII 111⊃33	XVIII 111Y⊃	III 111
CXC ⊃3333⊃1	CIII 111⊃1	XXXIV 1111⊃33	XIX 1111Y⊃	IV 1111
CC ⊃11	CIV 1111⊃1	XXXV Y⊃33	XX 33	V Y
CCC ⊃111	CV Y⊃1	XXXVI 1Y⊃33	XXI 133	VI 1Y
CCCC ⊃1111	CVI 1Y⊃1	XXXVII 11Y⊃33	XXII 1133	VII 11Y
D ⊃Y	CVII 11Y⊃1	XXXVIII 111Y⊃33	XXIII 1113	VIII 111Y
DC ⊃1Y	CVIII 111Y⊃1	XXXIX 1111Y⊃33	XXIV 1113	IX 1111Y
DCC ⊃11Y	CIX 1111Y⊃1	XL 33	XXV Y3	X ⊃
DCCC ⊃111Y	CX ⊃⊃1	L ⊃33	XXVI 1Y3	XI 1⊃
DCCCC ⊃1111Y	CXX 3⊃1	LX 333	XXVII 11Y3	XII 11⊃
M ⊃⊃1	CXXX ⊃3⊃1	LXX ⊃333	XXVIII 111Y3	XIII 111⊃
	CXL 33⊃1	LXXX 3333	XXIX 1111Y3	XIV 1111⊃
	CL ⊃33⊃1	XC ⊃3333	XXX ⊃3	XV Y⊃

Hence

Hence it appears, that the antient Palmyrenes, in this part of their notation, uſed only four numeral characters; two of which were letters of their alphabet, or at leaſt a ſort of marks endued with ſimilar forms. So that this kind of notation might have prevailed amongſt them ſeveral centuries before the commencement of the Chriſtian æra. For we learn from (*a*) Diodorus Siculus, that the Arabs of Petra, or Al Hejr, on the confines of the deſarts of Syria, and at no very great diſtance from the borders of Irâk, uſed the very ſame letters with thoſe of the neighbouring Syrians, and therefore probably of the people of Tadmor, 311 years before the birth of CHRIST. This gives us ſome reaſon to believe, conſidering the ſituation of the aforeſaid Arabs, that theſe letters could not have been very different from thoſe, which three or four centuries afterwards formed the alphabet of the Palmyrenes.

7. It ought to be obſerved, that the fourth inſcription is dated in the year of Seleucus 554, nearly coincident with the year of CHRIST 243, towards the cloſe of Gordian's reign; and conſequently before Philip's elevation to the imperial throne. This may be fairly collected from the ninth of Mr. Dawkins's Greek inſcriptions, as well as from that drawn in the language of the Palmyrenes, which I have here been endeavouring to explain. From hence likewiſe it ſeems plainly enough to appear, that the emperor Alexander Severus himſelf was at Tadmor in the

(*a*) Diod. Sic. *Bibliothec. Hiſtoric.* Lib. xix. p. 723. Edit. Rhodoman. Hanoviæ, 1604.

year

year of our LORD 233; when he marched againſt Artaxerxes, king of Perſia, overthrew him with great ſlaughter, and forced him, after he had raiſed the ſiege of Niſibis, to retire with ſhame into his own dominions.

8. I have taken the liberty to convert the ſixth letter in the ſecond line of the ſixth inſcription, which ſeems to reſemble *Mem* or *Koph*, into *Nun*; as ADINATH, or ODÆNATHVS, was the proper name of ſeveral great perſonages at Tadmor; whereas we have little reaſon to think, that ſuch a proper name as ADIMATH, ADICATH, ARIMATH, or ARICATH, was ever known amongſt the Palmyrenes.

9. The word MADA, or MADDA, ſeems to be the ſame proper name with MAΘΘA, or MATTHA, that occurs in the fifth of Dr. Bernard's Greek inſcriptions.

10. That the three words following ACTPATHΓA, in the fourth line of this inſcription (however diſfigured or defaced by time, or inaccurately taken) were originally what I have made them, is rendered probable by the mutilated words MAPTΥPHΘEN..., TEIMHCAMEN..., and BOΥAH.., exhibited, as well as the proper names CEAEΥKON and CEEIΛAΔΥ::::, by the correſpondent Greek inſcription.

11. With regard to the words שמש ושפר, which likewiſe preſent themſelves to our view in the ſixth inſcription, they cannot well admit of any other ſenſe, than that which I have aſſigned them. However, as they can receive no manner of illuſtration from the fragments of the correſpondent Greek one, I only offer this, as what appears to me at preſent the moſt probable; eſpecially, as the inſcription I am conſider-

ing ſeems to have ſuffered more from the injuries of time, than moſt of the others, which I have here been endeavouriug to explain.

12. The date of this inſcription falls in with the year of Seleucus 566, which nearly coincides with the 255th of CHRIST, about two years after Valerian was fixed upon the imperial throne.

13. With regard to the third inſcription, I ſhall firſt beg leave to remark, that in the age, in which it was written, the Palmyrenes ſeem to have been extremely fond of the letter *Vau*. This may be clearly evinced from the words חויהון and וחוי, written two hundred years afterwards חיוהון and וחיי, or וחיא, which appear towards the cloſe of the inſcription. Nor will this be any matter of ſurprize, when it is conſidered, that the Jews, whoſe language was then the Syriac, ſometimes uſed the epenthetic *Vau*, or a letter equivalent to it, a little before the year in which our inſcription was drawn; as we may infer from the words ELOI, ELOI, inſtead of the Hebrew ELI, ELI, ſpoken by our Saviour upon the croſs, juſt before he expired. That the mutilated letter beginning the eighth line of this inſcription was originally an *Aleph*, and that the word to which it belongs is to be deduced from the root סבת, or שבת, notwithſtanding the epenthetic ה, from whence, in the conjugation *Aphel*, is formed אסבית, or אסהבית, if the natural and genuine ſenſe of the paſſage be duly attended to, cannot, I think, be well denied (*a*).

(*a*) Paſor. *Lexic.* p. 652. Ed. Schoettgen. Lipſiæ, 1717. Schind. ubi ſup. p. 54. Buxtorf. *Gram. Chald. et Syr.* p. 36. Baſileæ, 1615.

13. The

14. The firſt word of the ſecond line here, which has been injured by time, was not improbably צאנה, or צנה, INFIXVM, ERECTVM, or COLLOCATVM EST; WAS FIXED, ERECTED, or PLACED; as the ſenſe of this part of the inſcription requires either that term, or one of a ſimilar ſignification. The chaſm at the end of the ſeventh line I ſhall not take upon me to fill up; tho' ſeveral words might be offered for this purpoſe, which would connect thoſe that precede and follow them, with very great propriety. That the ſtone containing the inſcription belonged formerly to an altar, is abundantly manifeſt from the word ועלתא, ET ARA or ALTARE, towards the cloſe of the ſecond line. Nor are we to be ſurprized, that ſeveral perſons are mentioned here, as concerned in the erection of it; ſince a ſimilar inſtance, if not one exactly parallel, preſents itſelf to our view, in the ſecond of Dr. Bernard's Greek Palmyrene inſcriptions. The firſt proper name, being imperfect, cannot ſo eaſily be made out; but the ſecond I take to be OMRIBOL SHEMS, or OMRIB OL SHEMS; which a pure and genuine Arab would have wrote AMRIO'L SHEMS, or AMRI AL SHEMS, and a Greek AMPICAMCOY. For, that this was one of the Palmyrene proper names, we learn from the fourth of Dr. Bernard's Greek Palmyrene inſcriptions; which exhibits it connected with IARIBOL, or IARIBOLIS, almoſt in the ſame manner as the monument at preſent under conſideration. And this circumſtance ought to be looked upon as an additional argument, in favour of what is here advanced. The next word ZEBID, or ZEBIDA, is likewiſe undoubtedly a Palmyrene proper name; as

moſt

moſt evidently appears from the thirteenth of the aforeſaid inſcriptions. The following term בנו, BENV, or BANV (agreeing with the *Arabic)* FILIORVM, relates to all the perſons before-mentioned. MALCHVS, IARIBOL, or IARIBOLIS, NASA, and MATTHA, are proper names of men, that occur in other Palmyrene inſcriptions. That the word ABDEBAL, i. e. THE SERVANT OF BAL, or BAAL, applied to MATTHA's father here as his proper name, ſavours very ſtrongly of the Eaſt, particularly of Syria, Phœnicia, and Arabia, where ſuch proper names as theſe were frequently aſſumed, is too clear and obvious a point to ſtand in need of any proof.

15. From the date prefixed to this inſcription we may conclude, that it is older than any of the others, handed down to us in the Palmyrene language and character, which have been hitherto publiſhed. For it is dated in the year of Seleucus 360, and conſequently was preceded by the commencement of the Chriſtian æra only 49 years, when the emperor Claudius preſided over the Roman world.

16. From the laſt obſervation we may collect, that one at leaſt of the inſcriptions, in the long portico, ſtill preſerved amongſt the ruins of Tadmor, is prior to the reign of the emperor Hadrian; though our celebrated Dr. Halley, who had not ſeen this, which I have now attempted to explain, took for granted, that all of them were poſterior to the commencement of that reign.

17. It has been thought proper here to add to the plate of Mr. Dawkins's inſcriptions, in the Palmyrene language and character, the two other plates, containing the Greek ones, with which he has favoured the public.

public. For, tho' few of the latter directly correſpond with the former; yet, by the affinity between ſeveral of the proper names, and the manner of expreſſion, wherein they apparently agree, to omit other circumſtances that occur, they greatly contribute to explain and illuſtrate one another.

Several other remarks on theſe noble remains of antiquity might here have been offered, and expatiated upon; which, for the reaſons already aſſigned, I muſt beg leave at preſent to ſuperſede. In the mean time, you will pleaſe to accept of my moſt grateful acknowledgments for the favour done me, and believe me to be, with all due ſentiments of reſpect,

SIR,

Your moſt obliged,

and moſt obedient ſervant,

J. Swinton.

LETTER III.

Reverend Sir, Chriſt-Church, Oxford, Sept. 2, 1754.

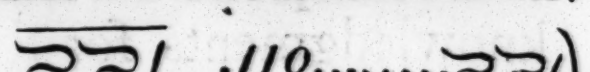
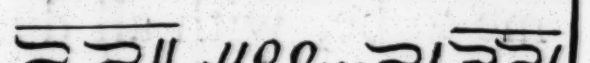
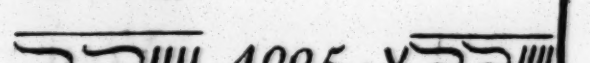
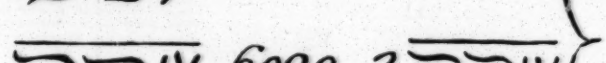
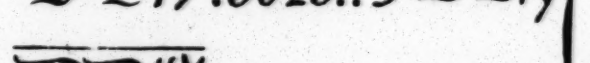
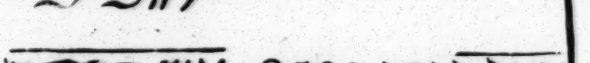
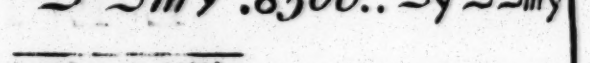
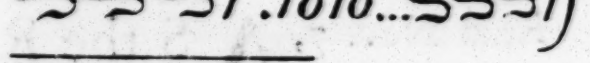
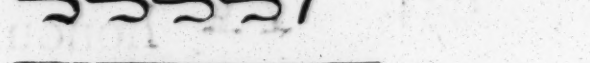
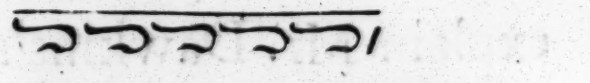
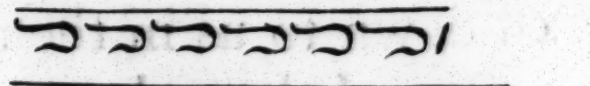
Read Nov. 7, 1754. AS the Royal Society have done me the honour to approve of my attempts to explain Mr. Dawkins's Palmyrene inſcriptions, handed down to us in the antient Chaldee, or Syriac, character; I have been thereby encouraged, before I conclude what I have to offer on that ſubject, to ſubmit to their ſuperior judgment the following additional obſervations.

1. Though my numeral table, from UNITY to a THOUSAND, may be intirely depended upon; yet I cannot, with the ſame certainty, extend this to the higher numbers.

numbers. However, as the Palmyrenes had no particular numeral character for an HUNDRED, as had the (1) Greeks and the Romans; from thence I conclude it probable, that they by no means expreſſed a THOUSAND by any ſuch character. If this be admitted, the following table of higher numbers may likewiſe be conſidered by the learned, as conſonant to the true and genuine notation of the Palmyrenes.

pag. 728.

PALMYRENE *Numerals from a* Thouſand *to a* Thouſand Millions.

1000	M	
2000	MM	
3000	MMM	
4000	MMMM	
5000	IↃↃ	
6000	IↃↃM	
7000	IↃↃMM	
8000	IↃↃMMM	
9000	IↃↃMMMM	
10000	CCIↃↃ	
100000	CCCIↃↃↃ	
1000000	CCCCIↃↃↃↃ	
10000000	CCCCCIↃↃↃↃↃ	
100000000	CCCCCCIↃↃↃↃↃↃ	
1000000000	CCCCCCCIↃↃↃↃↃↃↃ	

(1) Herodian. ab Hen. Stephan. & Joh. Scapul. ad *Lexic. Græc.* calcem edit. Sertor. Urſat. *De not. Romanor.* Johannis Wall. *Oper. mathematic.* Vol. prim. c. viii. p. 43—46. Oxon. 1695.

This

This ſeries, or mannner of numeral expreſſion, if agreeable to the learned, may with the ſame facility be continued *ad infinitum*.

2. The Palmyrene names of five of the twelve months occur in our inſcriptions. Theſe are *Pellul*, *Tebeth*, *Niſan*, *Elul*, and *Tiſri*. To which we may add from a Palmyrene inſcription, publiſhed by (2) M. Spon, and others, that of a ſixth, *viz*. *Shebat*, or *Shabat*, which has been applied to one of the Syrian Months by ſome of the moſt celebrated (3) chronologers. Theſe names point out to us ſix months anſwering, in ſome reſpects at leaſt, if not intirely, to the Syro-Macedonian months, *Apellæus*, *Aydynæus*, *Xanthicus*, *Gorpiæus*, *Hyperberetæus*, and *Peritius*; as moſt evidently appears from what has been obſerved by the (4) learned Dr. Fabricius, and the correſpondent Greek inſcriptions. But notwithſtanding the agreement here mentioned, that they did not however exactly coincide with the Syro-Macedonian months, we may perhaps be allowed to infer from the (5) firſt of Mr. Dawkins's Palmyrene inſcriptions; which ſeems to intimate, that the fourth day of *Tebeth* was the twenty-fourth of *Aydynæus*, and conſequently that the former began the twenty-firſt day of the latter month. And that this Palmyrene inſcription has

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(2) Jacob. Spon. *Miſcellan. Erudit. Antiquitat.* p. 1. Lugduni, 1685. Joan. Polen. *Utriuſq. Theſaur. Ant. Rom. & Græc. Nov. Supplement*, Vol. IV. p. 407, 650. Venetiis, 1737. Montfauc. *L'Antiquité Expliquée*, Tom. II. par. ii. p. 391. A Paris, 1719.

(3) Scalig. *De Emendat. Tempor.* p. 95, 350. Guliel. Bevereg. *Inſtitut. Chronologic.* p. 71. Lond. 1716. Jo. Albert. Fabric. *Menolog.* p. 20. Hamburgi, 1712.

(4) Jo. Albert. Fabric. ubi ſup. p. 20, 44.

(5) Dawk. *Marmor. Palmyren.* Inſcript. Palmyren. I, 8.

been rightly taken in the part under conſideration, may appear probable from hence, that the word יומם, or יומן, in the plural number, ſeems to be naturally connected with the numeral FOUR, and occurs (6) actually connected with that numeral in the Old Teſtament. Unleſs it ſhould be ſaid, that what now appears upon the face of the inſcription as the laſt letter *Nun* was originally, though altered by time, the Palmyrene numeral character expreſſing TWENTY; to which indeed at preſent it is not much unlike. Which if we admit, the word יום, in the ſingular number, muſt be allowed to have a connection here with the numeral TWENTY-FOUR. Nor are ſimilar inſtances of ſuch a connection difficult to be found. Nay, the very expreſſion ביום עשרים וארבעה. DIE VICESIMO QVARTO, ON THE TWENTY-FOVRTH DAY, preſents itſelf to our view HAG. i. 15. From whence ſome perſons will be apt to conclude, that nothing can be more juſt than the emendation here propoſed. It may not be improper to obſerve, that the Palmyrene inſcription publiſhed by Gruter and M. Spon, has (7) been preſerved on a marble, that formerly remained in the gardens belonging to Cardinal Carpegna, and afterwards in thoſe of the Princes Juſtiniani, near St. John Lateran, at Rome. I ſhall take the liberty to inſert here the Syrian, or Syro-Chaldæan, and Syro-Macedonian names of the aforeſaid months, in order the more clearly to point out the difference between them and thoſe of the Palmyrenes.

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(6) JUD. xi. 40.

(7) Montfauc. *L'Antiquité Expliquée*, Tom. II. par. ii. p. 391. A Paris, 1719.

Syro-

Syro-Chaldæan names.	Syro-Macedonian names.	Palmyrene names.
Canun prior.	*Apellæus.*	*Pellul.*
Canun poſterior.	*Aydynæus.*	*Tebeth.*
Shebat.	*Peritius.*	*Shebat.*
Niſan.	*Xanthicus.*	*Niſan.*
Eilul.	*Gorpiæus.*	*Elul.*
Tiſri.	*Hyperberetæus.*	*Tiſri.*

By the names *Tebeth* and *Elul* it ſhould ſeem, that the proper Syrians did not approach ſo near the Jews, in the appellations of their months, as did the Palmyrenes.

3. In order to ſet in a clearer light what has been here obſerved of the laſt-mentioned inſcription, I have been perſuaded by the Reverend and very Learned Mr. Sanford, Fellow of Balliol College, to attempt an explication of it; which I now beg leave to ſubmit to the judgment of our moſt illuſtrious Society. Nor ſhall I be greatly cenſured, as the forms of the letters, of which it is compoſed, have undoubtedly been altered by time; and as none of the copies of it, that I have ſeen, have been exactly taken; if this in all points ſhould not be ſtrictly conformable to truth. All of thoſe letters, as exhibited by the copies now before me, are not eaſily reducible to the correſpondent elements in Mr. Dawkins's Palmyrene alphabet, by the aſſiſtance of which I have endeavoured to decipher this inſcription. However, as no ſmall aſſiſtance has likewiſe been afforded me by the Greek one anſwering to it, and the numeral characters at the end of it, as well as by the ſecond, third, and thirteenth, of Mr.

Dawkins's Palmyrene inſcriptions, on which my conjectures have already been offered; I hope the following interpretation of it (which yet I would have only conſidered as an imperfect eſſay) will not prove unacceptable to the learned.

The Palmyrene Inſcription publiſhed by Gruter and M. Spon.

See Plate XXX, *Number* I.

לעגלבול ומלכבל ובמיתא די כספא ותצביתה עבד
מן כיסה ירחי בר חליבי בר
ירחי ברל שמש שער ועל היוהי והיא בנוהי
בירח שבט שנת דמז

AGLIBOLO ET MALACHBELO ET MONVMENTVM ELEVATVM (ſive SIGNVM) ARGENTI ET ORNATVM EIVS PARAVIT EX MARSVPIO SVO IARHÆVS (vel IARÆVS) FILIVS HALIBÆI (vel CHALIBÆI) FILII IARHÆI (vel IARÆI) PROPTER OFFICIVM (ſive RELIGIONEM) TIMORIS (i. e. EX VOTO A IARÆO IN ALIQVO PERICVLO CONSTITVTO ET TIMORE PERTERRITO FACTO) ET OB SALVTEM SVAM ET SALVTEM LIBERORVM (vel FAMILIÆ) EIVS MENSE SHEBAT ANNI DXLVII.

IARÆVS THE SON OF HALIBÆVS, or CHALIBÆVS, THE SON OF IARÆVS, DEDICATED THIS ELEVATED MONVMENT OF SILVER, AND ITS ORNAMENTS, PREPARED AT HIS OWN EXPENCE, TO AGLIBOLVS AND MALACHBELVS, IN CONSEQVENCE OF A VOW HE HAD MADE, WHEN IN GREAT FEAR AND DANGER, AND FOR THE SAFETY, HEALTH,

HEALTH, or PRESERVATION, OF HIMSELF AND HIS FAMILY, IN THE MONTH SHEBAT OF THE YEAR DXLVII.

4. That the letter ח, (8) in Hebrew, Syriac, Chaldee, and Phœnician words, ſometimes anſwered to the elements CH, or KH, ſometimes to H, and at other times was quieſcent, or endued with no power at all, we may fairly infer from ſeveral inſtances produced on this head by the famous M. Bochart. From whence I ſhall not ſcruple to conclude, that the Palmyrene proper names of men in this inſcription may be written in Latin either IARHÆVS, HALIBÆVS, and IARHÆVS; or IARÆVS, CHALIBÆVS, and IARÆVS: which if we admit, the firſt and laſt of them are the very ſame with one of thoſe, that occur in the (9) ſeventh of Dr. Bernard's Greek Palmyrene inſcriptions; and the other one very well known in the neighbourhood of Tadmor. The laſt point is clearly evinced by the word CHALEB, CHALIB, CHALIBON, or CHALYBON, the name of a city at no great diſtance from Tadmor, which has been mentioned by the (10) prophet EZEKIEL, Ptolemy, and Strabo.

5. That the language of this inſcription is the ſame, with that of the others copied by Mr. Dawkins, and conſequently little different from the pure Syriac,

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(8) Boch. *Phal.* Lib. III. c. xiv. p. 220. *Chan.* Lib. I. c. xxii. p. 502. c. xliv. p. 765. & alib. paſs.

(9) Edward. Bernard. *Monument. Palmyren.* Inſcript. vii. p. 4. Rotterædami, 1716.

(10) EZEK. xxvii. 18. Ptol. *Geogr.* Lib. v. c. 15. Strab. *Geogr.* Lib. xv.

will

will at firſt ſight appear to every one, who has been in the leaſt converſant with that dialect. However, that it contains one or two ſlight variations from the pure Syriac, cannot well be denied; ſince the words ברל שמש, for בריל שמוש, if rightly taken in Father Montfaucon's copy, which I have followed here, may not improperly be conſidered in the light of ſuch variations.

6. It appears from the matter of the inſcription now before us, that IARÆVS dedicated the monument therein mentioned to AGLIBOLVS and MALACHBELVS, the two moſt celebrated Palmyrene deities, in order to perform a vow he had made, when in great fear, and apprehenſive of ſome imminent danger; as likewiſe in order to avert both from himſelf, and his family, all future evils and dangers. The Palmyrene words therefore preſerved on the ſtone, that exhibits them, confirm what has been ſuggeſted by (11) M. Spon (which is not clearly deducible from the Greek words anſwering to them) in relation to the end, or deſign, of IARÆVS's dedication of the monument pointed out to us by this inſcription.

7. With regard to Mr. Dawkins's Palmyrene inſcriptions, that indicate altars, or other votive monuments, to have been erected either on account of deliverances from ſome impending dangers, in conſequence of vows formerly made; or for the preſervation of the perſons therein mentioned from future evils; nothing uncommon or extraordinary appears in them. Such inſcriptions as theſe were not only

(11) Jacob. Spon. ubi ſup. p. 2.

ſometimes drawn out by the Palmyrenes, as is abundantly evident from the third of (12) Dr. Bernard's Greek Palmyrene inſcriptions, as well as that I am here conſidering, and another publiſhed by (13) M. Spon; but likewiſe extremely common both amongſt the Greeks and Romans. In ſupport of which aſſertion a multitude of inſtances might be produced, did it ſtand in the leaſt need of them. But, as this is not the caſe, it will be ſufficient to refer the curious, for their farther ſatisfaction in this particular, (14) to the authors cited here.

8. As for the honorary inſcriptions, ſuch as are exhibited by ſome of the Palmyrene marbles, inſpected by Mr. Dawkins, they were ſtill, if poſſible, more common than the others, both amongſt the Greeks, the Romans, and the Palmyrenes. So common indeed, that whole volumes might be collected of them. Nay, many might judge too prolix a bare enumeration of even the names of all thoſe writers; though I ſhall take the liberty to mention (15) ſome of them here, who have publiſhed and attempted to explain them.

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(12) Edw. Bernard. *Monument. Palmyren.* Inſcript. III. p. 2.

(13) Jac. Spon. ubi ſup. p. 3.

(14) Gruter. & Reineſ. paſs. Baudelot. *De Util. Peregrin. in Diis Larib.* Spon. ubi ſup. p. 4. Prid. *Marm. Oxonienſ.* p. 282, 287. Oxon. 1676. Raph. Fabret. paſs. Joan. Oliv. Rhodigin. *in Marm. Iſiac. Exercitat.* C. iv. p. 23—33. & C. x. p. 70—82. Romæ, 1719. Lud. Ant. Murator. in *Nov. Theſaur. Vet. Inſcript.* Mediolani, 1739, 1740, 1742. *Marmor. Taurinenſ.* Auguſtæ Taurinorum, 1743, 1747.

(15) Gruter. Reineſ. Prid. Raph. Fabret. Edw. Bernard. *Monument. Palmyren.* Seller's *Append. to the Antiquit. of Palmyr.* Lud. Ant. Murator. ubi ſup. *Marmor. Taurinenſ.* &c.

9. Three

9. Three Palmyrene names of Syrian deities occur in Mr. Dawkins's inſcriptions, and that I am at preſent endeavouring to decipher; *viz.* IARIBOLVS, AGLIBOLVS, and MALACHBELVS of which the firſt points out to us the Moon, and the other two, according to ſome very learned writers, the Sun. That the firſt name was antiently applied by the Syrians and Palmyrenes to the *Deus Lunus*, or the Moon, is allowed on all hands; and has (17) been ſo clearly demonſtrated, that it does not ſtand in need of any farther proof. And that AGLIBOLVS, or AGLIBOL, was in early times one of the Syrian names of the Sun, who was dignified by his adorers with the title of MALAC, or MALEC, KING, is, I conceive, plain from hence, that the word (18) AGLIBAL, or AGLIBOL, apparently ſignifies THE ROVND LORD; which appellation could not ſo properly and emphatically have been applied to any other pagan divinity, as the ſun. The people of Tadmor wrote the laſt word in the compoſition of this name בעל, *Baal*, בל, *Bal*, or *Bel*, and בול, *Bol*, *Bul*, or *Pul*; as may be inferred from ſome of Mr. Dawkins's Palmyrene (19) inſcriptions, in conjunction with that I am now conſidering,

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(16) Jan. Gruter. *Corp. Inſcript. ex Recenſ. et Annotat.* Joan. Georg. Græv. p. lxxxvi. Amſtelædami, 1707. Joan. Selden. *De Diis Syr.* p. 226. Lipſiæ, 1668. Bochart. *Chan.* Lib. II. c. viii. p. 811. Francofurti ad Mœnum, 1681.

(17) Edw. Bernard. *Schol. ad Monument. Palmyren.* p. 21. Tho. Smith. *Annotat. in Monument. Palmyren.* p. 53. Rotteræ-dami, 1716. Cl. Halleius in *Act. Philoſoph. Anglican.* Vol. xix. n. 218. p. 171.

(18) Bochart. ubi ſup.

(19) Dawk. *Marm. Palmyren.* Inſcript. Palmyren. iv, 9. l. 6. Inſcript. Palmyren. iii. l. 5.

which

which has been publiſhed both by Gruter and M. Spon. I have taken no notice of the name IVPITER BELVS, though it occurs in the ſeventh of Dr. Bernard's Greek Palmyrene inſcriptions; becauſe that name, however the deity, to whom it appertained, might have been reverenced by them, did not properly belong to the Palmyrenes.

10. It has been remarked by (20) Mr. Seller, and may be collected from ſome of the (21) Palmyrene inſcriptions themſelves, that ſeveral of the moſt eminent citizens of Tadmor had both Syriac and Roman names. To which I would beg leave to add, that ſome of their aſſumed ſurnames ſeem to be deducible from the Greek language alſo; the words PHILINVS, SELEVCVS, *&c.* that occur in the fifth, ſixth, *&c.* of Mr. Dawkins's Palmyrene inſcriptions, being apparently ſuch ſurnames. And this is farther confirmed by the Palmyrene words now before me, and the correſpondent Greek inſcription; in the former of which the Palmyrene, whoſe memory has been perpetuated by the monument I am at preſent conſidering, is called IARHÆVS, or IARÆVS, and in the latter, T. AVRELIVS HELIODORVS. But, for a more particular account of the different appellations of the Palmyrenes, I muſt refer the curious to the writer (22) laſt mentioned here.

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(20) Seller's *Append.* c. ii. p. 187, 188, 189, 190, &c. Lond. 1705.

(21) Dawk. *Marm. Palmyren.* Inſcript. Palmyren. iv, 9. viii, 16. ix, 17. x, 19. &c.

(22) Seller's *Crit. Obſervat. &c.*

11. It will at firſt ſight be allowed, that the forms of ſeveral of the letters, exhibited by the inſcription I am attempting to illuſtrate, are conſiderably different from thoſe of the letters preſerved by Mr. Dawkins's Palmyrene inſcriptions; tho' the forms of every particular element repeated in the firſt of theſe monuments, at leaſt if we may depend upon F. Montfaucons's copy, nearly reſemble one another. From whence I would infer, that the letters of M. Spon's inſcription were drawn at Rome, either by a perſon not ſufficiently acquainted with the true forms of the Palmyrene letters; or by a native of ſome part of Syria, where an alphabet was uſed not exactly the ſame with that of the Palmyrenes. The latter of which ſuppoſitions ſeems to me at preſent the more probable. I ſhall therefore, in conſequence of this opinion, take for granted, at leaſt till I can meet with a more accurate copy of the Roman Palmyrene inſcription, that the alphabet, to which its elements belonged, was ſomething different from that of the Palmyrenes; though I ſhould not be ſuprized, if hereafter, upon an inſpection of that inſcription, thoſe two alphabets ſhould appear to be intirely the ſame. Nor will this ſeem altogether improbable, if it be conſidered, that (23) one of Mr. Dawkins's Palmyrene inſcriptions is only three years earlier, and (24) another of them not above ſeven years later, than the monument I have now in view. What is here advanced will perhaps be thought not ſo remote from

(23) Dawk. *Marm. Palmyren.* Inſcript. Palmyren. i, 8.
(24) Dawk. *Marm. Palmyren.* Inſcript. Palmyren. iv, 9.

truth,

truth, if we admit, what has been ſuggeſted by two (25) very great men; *viz.* that this marble was brought to Rome from Tadmor by the emperor Aurelian himſelf, after he had pillaged the latter of thoſe cities, and treated its inhabitants with uncommon cruelty, according to one of his own letters, ſtill extant in (26) Vopiſcus. Which notion if we think fit to adopt, we muſt likewiſe allow, that the two deities exhibited by this ſtone were BAAL, BELVS, or IVPITER, and the SVN; thoſe having been the deities, according to the authors who have eſpouſed this notion, as well as (27) Zoſimus, and not Herodian, as Dr. Hyde (28) aſſerts, whoſe images were carried from Tadmor to Rome by that emperor.

After what has been obſerved on this head, I ſhall perhaps be excuſed, if I lay before the Society the following alphabet, deduced from the Roman Palmyrene inſcription publiſhed by Gruter and M. Spon.

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(25) Selden. ubi ſup. Tho. Hyd. *Hiſt. Rel. Vet. Perſ.* p. 116, 117. Oxon. 1700.

(26) Flav. Vopiſc. in *Aurelian.*

(27) Zoſim. Lib. i.

(28) Hyd. ubi ſup.

pag. 740.

The PALMYRENE *Alphabet, according to the Inſcription published by* Gruter *and* Spon.

	Palmyr.	Hebr.		Palmyr.	Hebr.
Aleph	אא	א	*Lamed*	ל	ל
Beth	בבבבב	ב	*Mem*	מת	מ
Gimel	ג	ג	*Nun*	נ	נ
Daleth	ד	ד	*Samech*	ככ	ס
He	הההה	ה	*Ajin*	ע	ע
Vau	ו	ו	*Pe*	פ	פ
Zain		ז	*Tzade*	צ	צ
Hheth	חחחח	ח	*Koph*		ק
Teth	ט	ט	*Resch*	ר	ר
Jod	יכ	י	*Schin*	ש	ש
Caph	כככ	כ	*Thau*	תתת	ת

It may not be improper to obſerve, that the forms of the *Nun* and the *Ajin*, in this alphabet, approach nearer the modern Syriac forms of thoſe letters, if they do not intirely agree with them, than they do thoſe of the ſame elements in the proper alphabet of the Palmyrenes.

12. With

12. With regard to the numeral characters, ſtill viſible on the Roman marble, it may not be amiſs to remark, that the laſt of them ſeems to have been altered by time. It was probably at firſt two ſhort ſtrait lines, which expreſs the number Two in ſome of (29) Mr. Dawkins's Palmyrene inſcriptions. If this ſhould be admitted by the members of our moſt learned Society, they will not be diſpleaſed to ſee the following numeral table, deduced from the inſcription I have been conſidering, from UNITY to a THOUSAND, not a little reſembling that of the true and proper Palmyrenes.

pag. 741.

PALMYRENE *Numerals from* One *to a* Thouſand, *according to the Inſcription publiſh'd by* Gruter.

D	𐡃	XL	𐡊𐡊	XXI	𐡉𐡊	XI	𐡉𐡃	I	𐡉
DC	𐡃𐡉	L	𐡃𐡊𐡊	XXII	𐡉𐡉𐡊	XII	𐡉𐡉𐡃	II	𐡉𐡉
DCC	𐡃𐡉𐡉	LX	𐡊𐡊𐡊	XXIII	𐡉𐡉𐡉𐡊	XIII	𐡉𐡉𐡉𐡃	III	𐡉𐡉𐡉
DCCC	𐡃𐡉𐡉𐡉	LXX	𐡃𐡊𐡊𐡊	XXIV	𐡉𐡉𐡉𐡉𐡊	XIV	𐡉𐡉𐡉𐡉𐡃	IV	𐡉𐡉𐡉𐡉
DCCCC	𐡃𐡉𐡉𐡉𐡉	LXXX	𐡊𐡊𐡊𐡊	XXV	𐡋𐡊	XV	𐡋𐡃	V	𐡋
DCCCCXX	𐡊𐡃𐡉𐡉𐡉𐡉	XC	𐡃𐡊𐡊𐡊𐡊	XXVI	𐡉𐡋𐡊	XVI	𐡉𐡋𐡃	VI	𐡉𐡋
DCCCCXL	𐡊𐡊𐡃𐡉𐡉𐡉𐡉	C	𐡃𐡉	XXVII	𐡉𐡉𐡋𐡊	XVII	𐡉𐡉𐡋𐡃	VII	𐡉𐡉𐡋
DCCCCLX	𐡊𐡊𐡊𐡃𐡉𐡉𐡉𐡉	CC	𐡃𐡉𐡉	XXVIII	𐡉𐡉𐡉𐡋𐡊	XVIII	𐡉𐡉𐡉𐡋𐡃	VIII	𐡉𐡉𐡉𐡋
DCCCCLXXX	𐡊𐡊𐡊𐡊𐡃𐡉𐡉𐡉𐡉	CCC	𐡃𐡉𐡉𐡉	XXIX	𐡉𐡉𐡉𐡉𐡋𐡊	XIX	𐡉𐡉𐡉𐡉𐡋𐡃	IX	𐡉𐡉𐡉𐡉𐡋
M	𐡃𐡃𐡉	CCCC	𐡃𐡉𐡉𐡉𐡉	XXX	𐡃𐡊	XX	𐡊	X	𐡃

(29) Dawk. *Marm. Palmyren.* Inſcript. Palmyren. iv, 9. v, 11. vii, 13. &c.

This

This table, if the manner of numeral expreſſion offered to the conſideration of the Society in the beginning of this letter be allowed, may be continued *ad infinitum*.

13. It is worthy obſervation, that the word בנוהי; HIS SONS, in the Roman Palmyrene inſcription, denoted IARÆVS's whole family, and conſequently included both the wife (as manifeſtly appears from the Greek inſcription anſwering to it) and the daughters (if he had any) of that Palmyrene. From whence we may conclude, that this word was pretty frequently uſed in ſuch an extenſive ſignification, about the middle of the third century after CHRIST, and probably much (30) earlier, at leaſt in ſome parts of the Eaſt.

As I have expatiated ſo largely upon Gruter's Palmyrene inſcription, with which perhaps it may be thought I was not at firſt ſo immediately concerned, I am afraid you will eſteem me too prolix. I ſhall therefore only beg leave farther to obſerve, that ſufficient acknowlegments can never be made to Mr. Dawkins, for the honour he has done his country, and ſervice to the republic of letters, in copying ſo exactly ſuch a number of inſcriptions, which were before intirely loſt to the learned world; and to aſſure you that I am, with the higheſt regard,

SIR,

Your moſt obliged,

and moſt obedient humble ſervant,

John Swinton.

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(30) Val. Schind. *Lex. Pentaglot.* p. 211. Hanoviæ, 1612.

LET-

LETTER IV.

Reverend Sir, Chriſt-Church, Oxford, Oct. 10, 1754.

Read Nov. 14, 1754. NOT long after I had finiſhed my conjectures upon the Palmyrene inſcription publiſhed by (1) Gruter and (2) M. Spon, I received a moſt obliging letter from M. l'Abbé Barthelemy, member of the Royal Academy of Inſcriptions and Belles-Letters at Paris, and keeper of the medals in the French king's cabinet; wherein he informed me, that he had taken great pains to explain that inſcription, and another drawn in the ſame character, publiſhed likewiſe by (3) M. Spon. As he ſeemed to think, that he had not intirely deciphered thoſe inſcriptions, he recommended it to me to take them both into my conſideration, and try what I could make of them. I received alſo, at the ſame time, from that very learned and polite gentleman, as a preſent, a copy of his reflections upon the alphabet and language of Palmyra; which, eſpecially as the piece itſelf is the reſult of great ſagacity and erudition, I eſteem as an additional favour. M. l'Abbé Barthelemy acquainted me in his letter, that he had procured a moſt faithful and exact copy of the ſecond Roman Palmyrene inſcription, which enabled him to expreſs it by the ſquare, or Chaldee,

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(1) Jan. Gruter. *Corp. Inſcript. ex Recenſ. & Annotat.* Joan. Georg. Græv. p. lxxxvi. Amſtelædami, 1707.

(2) Jac. Spon. *Miſcellan. Erudit. Antiquitat.* p. 1. Lugduni, 1685.

(3) Idem ibid. p. 3.

letters

letters exhibited in his third plate. It may not be improper to obſerve here, that we are obliged for the publication of this laſt inſcription to (4) F. Montfaucon, (5) Mr. Reland, and (6) Sig. Poleni, as well as to M. Spon.

In my anſwer of the 8th inſt. to M. l'Abbé's letter, dated at Paris, Sept. 5th, which reached me here the 12th of the ſame month, I told him, that I had drawn out my conjectures upon the firſt of the inſcriptions he deſired me to give ſome attention to, before the arrival of his letter; but that I could not at preſent find time to attempt an explication of the other. Nor indeed ought I to think of undertaking ſuch a taſk, before I can procure an accurate copy of the inſcription to be explained, which M. l'Abbé Barthelemy ſays he is in poſſeſſion of; thoſe to be met with in F. Montfaucon, Mr. Reland, Sig. Poleni, and M. Spon, being ſo inaccurately taken, that, with regard to the forms of the letters they exhibit, they cannot certainly be depended upon.

If the ſquare, or Chaldee, letters at the bottom of M. l'Abbé's third plate do really anſwer to the Palmyrene elements, they are intended to repreſent; the SVN had the name, or ſurname, of MALACHBELVS, MALACBELVS, or MALECBAL, that is to ſay, KING BAAL, given him by the Palmyrenes. Which

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(4) Montfauc. *L'Antiquit. Expliq.* Tom. II. par. ii. p. 392. pl. clxxix.

(5) Hadrian. Reland. *Palæſt. Illuſtrat.* Tom. ii. p. 526. Trajecti Batavorum, 1714.

(6) Joan. Polen. *Utriuſq. Theſaur. Antiquitat. Romanar. & Græcar. Nov. Supplement.* Vol. quart. p. 411, 654. Venetiis, 1737.

it

if we admit, the names, or ſurnames, of AGLIBOL, or AGLEBAL, and MALECBAL, may be conſidered as two different titles, or ſurnames, of the SVN; or rather as two appellations pointing out to us two pagan divinities, with which the Palmyrenes were ſupplied by the different appearances, or influences, of the Sun in ſummer and winter. This notion has been countenanced (7) by Salmaſius, and ſome (8) other very learned men. However, I muſt frankly own, that I cannot, to my intire ſatisfaction, yet trace out the elements M. l'Abbé has favoured us with, in any of the copies of this inſcription, that has hitherto fallen under my inſpection.

That IVPITER and the SVN were looked upon as the ſame deity, and denominated BAAL, or BAAL SHEMESH, by the people of Abila, a town of Syria, at no very great diſtance from the famous Heliopolis, if not by the Palmyrenes; we may, I think, fairly infer from an inſcription in the Palmyrene character, copied by (9) Signiore Pietro della Valle, (10) at Teive, or Teibe, between two and three days (11) journey from Tadmor, in conjunction with a (12) Greek one publiſhed by Dr. Bernard, that appears upon the ſame ſtone. The inſcription in the Palmyrene cha-

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(7) Claud. Salmaſ. ad Flav. Vopiſc. in *Div. Aurelian.*

(8) Jan. Gruter. ubi ſup. Joan. Selden. *De Diis Syr.* Syntag. ii. p. 226. Lipſiæ, 1668. Bochart. *Chan.* p. 811. Francofurti ad Mœnum, 1681.

(9) Hadr. Reland. ubi ſup. p. 525, 526.

(10) *Philoſoph. Tranſact.* Vol. xix. n. 217. p. 109. & n. 218. p. 173.

(11) *Philoſoph. Tranſact.* Vol. xix. n. 217. p. 109.

(12) Edw. Bernard. *Monument. Palmyren.* p. 2. Rotterædami, 1716.

racter, as to the forms of ſome of its letters, has either been a little inaccurately taken by Signiore Pietro della Valle himſelf; or a little inaccurately copied by Mr. Maſſon, from that gentleman's original papers. However, as the words AGATHAGELVS, or AGATHANGELVS, ABILENVS, BAAL, and SHEMESH, therein plainly enough occur; I would, if ſuch a liberty might be indulged me by the learned, willingly beſtow the following Latin and Engliſh verſions upon the inſcription now in view.

See Plate XXX, *Number* II.

לבעל שמש מר אבלנוס מסב
סבהאר עשא אגתגלס

BAAL SHEMESH (BELO SOLI vel DOMINO SOLI) DEO ABILENO DOMICILIVM (HABITATIONEM vel MANSIONEM) INCOLVMITATIS (vel SALVTIS) FECIT AGATHANGELVS.

AGATHANGELVS PREPARED A PLACE OF SAFETY, or HEALTH, FOR BAAL SHEMESH, THE SVN, or IVPITER THE THVNDERER, THE GOD OF THE ABILENES.

That the inſcription, according to this interpretation, ſufficiently expreſſes the ſenſe of the Greek one anſwering to it, though in a very conciſe manner, will not, I believe, be denied; which may be conſidered as a pretty ſtrong preſumption, that my explication of it cannot be very remote from truth. But this will be ſtill more evident from a cloſer atten-

attention to the particular terms of which it is compoſed. The ſignification of the two firſt words לבעל שמש, BAAL SHEMESH, or DOMINO SOLI, cannot well be miſtaken. That the third word מר, MAR, may be tranſlated into Latin either (13) DOMINO or DEO, will by every orientaliſt be eaſily allowed. The fourth manifeſtly appears to be אבלנוש, ABILENVS, or in Latin by appoſition ABILENO; though the ſecond, fourth, and fifth, of its letters have been ſomething altered and effaced. The fifth מסב may be looked upon as the ſame with מושב, or משב, (14) DOMICILIVM, HABITATIONEM, MANSIONEM, &c. the Syrians (15) ſometimes ſuppreſſing *Vau*, and uſing *Samech* for *Schin*. The ſixth סבהאר, or (16) סבהור, anſwers to the Latin INCOLVMITATIS, SALVTIS, &c. The ſeventh עשא apparently denotes (17) FECIT, PERFECIT, ABSOLVIT, &c. the Syrians not ſeldom converting the Hebrew *He* in (18) verbs of this kind into *Aleph*. And the eighth, if

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(13) Edm. Caſtel. *Lex. Heptaglot.* p. 2128. Val. Schind. *Lex. Pentaglot.* p. 1034, 1035.

(14) Edm. Caſt. ubi ſup. p. 1648. Val. Schind. ubi ſup. p. 814, 815.

(15) See the firſt Roman Palmyrene inſcription, as above explained. Schind. ubi ſup. p. 1826, & alib.

(16) Vid. *Targ.* & *Verſ. Syriac.* in PROV. ii. 7. & Caſtel. ubi ſup. p. 2450.

(17) Caſtel. ubi ſup. p. 2928, 2929. Schind. ubi ſup. p. 1399, 1400.

(18) Joh. Buxtorf. *Theſaur. Gram. Hebr.* p. 251, 252. Baſileæ, 1663. Buxtorf. *Gram. Chald. et Syriac.* p. 161. Baſileæ, 1615. Schind. ubi ſup. p. 313, 314, 315, &c.

an allowance be made for a ſmall alteration in the ſecond and ſixth letters, will be אגתגלס, AGATHAGELVS, AGATHAGGELVS, or, as the Greeks pronounced, and the Romans wrote it, AGATHANGELVS; the Greek *Nu* being (19) ſometimes omitted in eaſtern words, and ſometimes ſupplying the (20) place of *Dageſch Forte*. Whence we may conclude, that the Abilenes, who were neighbours to the Heliopolitans, if not the people of Tadmor, took the Greek ΖΕΥΣ ΚΕΡΑΥΝΙΟΣ, the Roman IVPITER FVLGVRATOR, or IVPITER THE THVNDERER, and BAAL SHEMESH, or the SVN, for the ſame deity; and conſequently that whether we look upon the words AGLIBOLVS and MALACHBELVS as pointing out to us IVPITER, or BELVS, and the SVN, (21) as ſome imagine, or only the latter of thoſe deities, as others (22) will have it, we cannot greatly err. From the inſcription now before us we may likewiſe collect, that ſome of the Syrians, inhabiting diſtricts not in the neighbourhood of Tadmor, made uſe of the very ſame letters, or alphabet, about the year of our LORD 134, which then prevailed amongſt the Palmyrenes.

What has been here advanced, in relation to the identity of the Greek ΖΕΥΣ, the Roman IVPITER, and BAAL SHEMESH, or the SVN, is perfectly conſonant to the ſentiments of Macrobius on this

(19) Bochart. *Chan.* p. 833.
(20) Idem ibid. & p. 534.
(21) Zoſim. Lib. i. Tho. Hyd. *Hiſt. Rel. vet. Perſ.* p. 117.
(22) Jan. Gruter. & Joan. Selden. ubi ſup. Sam. Bochart. ubi ſup. p. 811.

head; who expreſly (23) affirms, that the Greek ΖΕΥΣ, the Roman IVPITER, the principal god of the Heliopolitans, and therefore undoubtedly that of their neighbours the Abilenes, were the very ſame deity with the SVN. *Nec ipſe Jupiter*, ſays that writer, *rex deorum, Solis naturam videtur excedere; ſed eumdem eſſe Jovem ac Solem claris docetur indiciis.* In ſupport of which aſſertion he cites Homer, Plato, Cornificius, Poſidonius, and Cleanthes; who, according to him, abſolutely evince ſuch a notion. The Aſſyrians alſo, or Syrians, if we will believe (24) him, allowed the IVPITER of the Heliopolitans, and therefore in all likelihood the IVPITER of their neighbours the Abilenes, to have been BAAL SHEMESH, or the SVN. *Aſſyrii quoque*, adds he, *Solem ſub nomine Jovis* (i. e. BAAL, BAALIS, vel BELI), *quem* DIA HELIOPOLITEN *cognominant, maximis cæremoniis celebrant in civitate, quæ Heliopolis nuncupatur.* Nay, which is directly in point, from the ſame author it very clearly (25) appears, that IVPITER HELIOPOLITES was not only BAAL SHEMESH, or the SVN, but likewiſe, in conformity to our inſcriptions, the ΖΕΥΣ ΚΕΡΑΥΝΙΟΣ, or IVPITER THE THVNDERER, of the Greeks. *Hunc vero*, continues he, *eumdem Jovem Solemque eſſe cum ex ipſo ſacrorum ritu, tum ex habitu dinoſcitur. Simulacrum enim aureum ſpecie imberbi inſtat dextra elevata*

(23) Macrob. *Saturn.* Lib. I. c. xxiii. p. 215—217. Lond. 1694.
(24) Idem ibid.
(25) Idem ibid.

CUM

CVM FLAGRO IN AVRIGÆ MODVM; *læva tenet* FVLMEN *et ſpicas, quæ cuncta Jovis Soliſque conſociatam potentiam monſtrant.* Sanchoniatho, Philo Byblius, Servius, Herodian, Julius Capitolinus, and others, in effect likewiſe aſſert the (26) ſame thing. And that the Sun was antiently worſhiped in thoſe parts of Syria, bordering upon Palæſtine, under the name of *Shemeſh*, or *Baal Shemeſh*, is ſufficiently implied by the local names *Beth Shemeſh*, *the temple of Shemeſh*, or the Sun; *Ir Shemeſh*, *the city of Shemeſh*, or the Sun; *Har Shemeſh*, *the mountain of Shemeſh*, or the Sun, &c. which (27) not ſeldom occur in Scripture. It has been remarked by Salmaſius (28), that ſeveral nations and cities of the Eaſt paid divine honours to the Sun under different appellations, each of them having one or more peculiar to itſelf. The people of Tadmor gave him the ſurnames of *Aglibolus*, and *Malachbelus*, as has been already obſerved; the citizens of Emeſa, or Hems, called him *Alagabalus*, *Elagabalus*, or *Heliogabalus*; the Perſians denominated him *Miher*, or *Mihr*, which was converted into *Mithra* by the Greeks; the Babylonians dignified him with the title of *Baal*, or *Bel*, importing *Lord*; and the Abilenes, according to the inſcription I have been endeavouring to decipher, *Baal Shemeſh*. With regard to the nature of the manſion, chamber, or apartment, erected and fitted up by AGATHANGELVS for BAAL SHEMESH, the SVN,

(26) Bochart. ubi ſup. p. 736, 737.
(27) Matth. Hiller. *Onomaſt. Sacr.* p. 771, 772. Tubingæ, 1706.
(28) Claud. Salmaſ. ubi ſup.

or

or IVPITER THE THVNDERER, I muſt refer the curious to what has been ſaid by (29) Mr. Seller, and the authors cited by him, on this head; as I cannot, conſiſtently with the brevity here propoſed, at preſent expatiate farther upon it. To what has been advanced, in order to illuſtrate this inſcription, which has never hitherto been explained, ſeveral other particulars, perhaps not altogether unworthy the attention of the learned, might have been added, were it not for the reaſon juſt hinted at. For as I may probably be thought to have treated the point before me in too copious, or rather prolix, a manner; it might be deemed improper to offer any farther obſervations at this time relative to it. I ſhall therefore only intreat you to believe me to be, with the greateſt reſpect and eſteem,

SIR,

Your moſt obliged,

and moſt obedient ſervant,

John Swinton.

(29) Sel. ubi ſup. p. 364—369.

ERRATA.

P. 724. l. 13, 14. read— יהוה and יהוא,

P. 726. l. 3. immediately after the parentheſis, read— unleſs we ſuppoſe it to have been originally בנו,

LETTER

LETTER V.

Reverend Sir, Chriſt-Church, Oxford, Oct. 21, 1754.

Read Nov. 14, 1754. IN my laſt letter I intimated to you, that I had no intention to offer any conjectures, at leaſt for the preſent, upon the ſecond Roman Palmyrene inſcription, publiſhed by F. Montfaucon, Mr. Reland, Sig. Poleni, and M. Spon, for the reaſon therein aſſigned. But having ſince been prompted by my curioſity to compare Mr. Reland's copy of it, by the aſſiſtance of the two Palmyrene alphabets I imagine myſelf to have diſcovered, with the Chaldee letters, that appear at the bottom of M. l'Abbé Barthelemy's third plate; I find that the latter may be ſuppoſed tolerably well to repreſent the former, and from thence am induced to conclude, that M. l'Abbé's copy of the ſame inſcription muſt have been pretty accurately taken. This has excited me to attempt, with all the attention I am capable of, an explication of that inſcription; the reſult of which I now do myſelf the honour to ſend you, drawn up in the ſhorteſt, and moſt conciſe, manner poſſible.

The inſcription then, after the introduction of an emendation in the ſecond line, and of two additional letters in the third, which I would recommend to the conſideration of the learned, may perhaps be pretty eaſily exhibited in the Chaldee character, and demonſtrated to expreſs the ſenſe of the Latin inſcription, with which it is ſuppoſed to correſpond.

The

The ſecond Roman Palmyrene inſcription, publiſhed by Mr. Reland, from a copy taken of it by Mr. Maſſon, in his *Palæſt Illuſtrat.* Tom. ii. p. 526. Trajecti Batavorum, 1714.

See Plate XXX, *Number* III.

עלתא דה למלכבל ולאלהי תדמר
קרב טבריס כלודיס כלביא
ותדמריא לאלהיה(ו)ן שלמ(ו)

ARAM HANC MALACHBELO ET DIIS TADMOR OBTVLIT (vel DEDICAVIT) TIBERIVS CLAVDIVS CALBIENSES ET TADMORIENSES (vel PALMYRENI) DIIS SVIS VOTVM SOLVERVNT.

TIBERIVS CLAVDIVS DEDICATED THIS ALTAR TO MALACHBELVS AND THE GODS OF TADMOR: THE CALBITES AND THE TADMORITES (or PALMYRENES) HAVE PERFORMED THE VOW THEY MADE TO THEIR GODS.

The terms, which form this inſcription, are ſo clear and intelligible, that it would be ſuperfluous to expatiate upon them. My converſion of M. l'Abbé Barthelemy's dubious *Samech* and *Jod*, the powers of which he ſeems not abſolutely to have aſcertained, into *Jod* and *Aleph*, in the word כלביא, at the end of the ſecone line, is ſufficiently juſtified, and even rendered inconteſtable, by the word ותדמריא, that immediately follows, and the correſpondent Latin

 inſcription.

inſcription. That part of the ſeventh letter in לאלהיה(ו)ן has been connected with the preceding element, and the other part defaced by the injuries of time, at firſt ſight ſeems clearly enough to appear; though, had this not been the caſe, we may eaſily conceive, from what has been already obſerved, that the Palmyrenes ſometimes in the affix הון might have omitted the letter (1) *Vau*. The pronoun (2) דה, or (3) דא, (not זה, (4) which is undoubtedly *Hebrew*) and the words עלתא קרב כלביא ותדמריא לאלהיה(ו)ן are all of them of the pure Syriac form. As for טברים כלודים, TIBERIVS CLAVDIVS, ſeveral ſimilar inſtances of ſuch Roman proper names, written in the Palmyrene character, have already (5) been produced.

That the laſt word was originally שלמו, SOLVERVNT, or rather VOTVM SOLVERVNT, HAVE PERFORMED THE VOW, as I have taken the liberty to tranſlate it here, is clearly evinced by the latter part of the Latin inſcription; with which this tranſlation ſo exactly correſponds. The term שלמו, as confined to the ſignification now aſſigned it, is apparently (6) Syriac; and occurs both in the (7) Old and New Teſtament. Hence we may conclude, that TIBERIVS CLAVDIVS, by the dedication of this altar, enabled ſeveral of the CALBITES and TADMORITES, who acted under his command, to

(1) See my *Third Letter*, p. 734. l. 5.
(2) Edm. Caſtel. *Lex Heptaglot.* p. 631, 632. Lond. 1669.
(3) Idem. ibid.
(4) Val. Schind. *Lex. Pentaglot.* p. 467. Hanoviæ, 1612. Caſtel. ubi ſup. p. 1019.
(5) See my *Firſt* and *Second Letters*.
(6) Edm. Caſtel. *Lex. Heptaglot.* p. 3766.
(7) PSAL. xxii. 25. MATTH. v. 33.

perform

perform or accompliſh a vow they had made; and that he conducted ſome important enterprize, which had occaſioned that vow, with great ſucceſs. Whether the principal figure on that ſide of the ſtone, which exhibits the Palmyrene inſcription, was intended to repreſent the SVN (8), as F. Montfaucon and others have aſſerted; or TIBERIVS CLAVDIVS himſelf, as ſome perhaps may ſuppoſe not leſs probable; I ſhall not pretend to decide: but that the inference here deduced from the term, which I have now been conſidering, can by no means be deemed unjuſt, will, I flatter myſelf, by the learned be very readily allowed.

Who the CALBITES mentioned in this inſcription were, we cannot ſo eaſily learn from any of the Greek or Latin authors. However, I make not the leaſt doubt, but they muſt have belonged to the Calbites taken notice of by (9) Abulfeda, the celebrated Arabian hiſtorian; who, according to that writer, were a tribe of Arabs that acknowledged for their great progenitor Calb Ebn Wabra, deſcended in a right line from Hamyar, the ſon of Saba, the fifth of the antient kings of Yaman. This tribe, in the times of ignorance (10), that is to ſay, before the introduction of Iſlamiſm into Arabia, occupied Dawmat Al Jandal, Tabûc, and ſeveral other places upon the confines of Syria. From whence, in conjunction with the inſcription now before me, which perhaps was brought to Rome from Tadmor by the emperor Aurelian himſelf, we may infer, that

(8) Montfauc. *L'Antiq. Expliq.* Tom. II. par. ii. p. 391, 392.

(9) Iſm. Abulfed. in cap. *De Arab. pur.* Poc. *Not. in Spec. Hiſt. Arab.* p. 40, 41. Oxon. 1650.

(10) Al Kâdi Saed Ebn Ahmed Andaloſen. apud Greg. Abul Faraj. in *Hiſt. Dynaſt.* p. 159. Oxon. 1663.

the Calbites, before the birth of Mohammed, probably extended themſelves from Dawmat Al Jandal and Tabûc to the borders of Palmyrene, and were even poſſeſſed of ſome diſtricts in the neighbourhood of Tadmor. According to the correſpondent (11) Latin inſcription, the TADMORITES, or PALMYRENES, as well as the (12) CALBITES, mentioned here to have expreſſed ſuch a regard for their local or tutelary deities, muſt have been ſoldiers belonging to the third cohort of one of the Roman legions, then quartered either at Tadmor, or ſome other place in the territories of the Palmyrenes. But who TIBERIVS CLAVDIVS was, what command he had aſſigned him in the Roman forces, or even at what time preciſely he lived, for want of ſufficient light from antient hiſtory, I cannot at preſent take upon me to determine.

Thus, Sir, you have my thoughts upon the ſecond Roman Palmyrene inſcription, the only one of thoſe hitherto publiſhed, which I have not already touched upon, laid before you with all poſſible brevity. For the reſt, I remain, with the higheſt reſpect and eſteem, SIR,

Your moſt obedient humble ſervant,

John Swinton.

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(11) Jacob. Spon. *Miſcellan. Erudit. Antiquitat.* p. 3. Lugduni, 1685.

(12) It appears from Eutychius, that the Calbites acknowledged the ſovereignty of the emperor Heraclius, and that even part of a body of troops, aſſembled by that prince to oppoſe the Moſlem forces, was drawn out of their tribe, in the Khalifat of Abu Becr. Eutych. *Annal.* Tom. ii. p. 270—273. Oxon. 1656.

LXXXVIII. *Extract of a Letter from* John Lining, *M. D. of* Charles Town, *in* South Carolina, *to* Charles Pinckney, *Esq*; *in* London: *With his Answers to several Queries sent to him concerning his Experiment of Electricity with a Kite.*

Charles Town, January 14, 1754.

Read May 4, 1754. INcloſed I ſend you anſwers to the queries, which you ſent me concerning the experiment with the kite.

Since I made that experiment laſt May, I have not had an opportunity of making any more, having been confined all the ſummer and autumn with the gout, which perhaps prevented my meeting with the ſame unhappy fate with profeſſor Richman. In the London Daily Advertiſer, Thurſday 27 September laſt, there is a more particular account of that unhappy accident. From that account it appears, that the profeſſor had a wire, which came down from the iron rod, erected on his houſe, through the gallery-ceiling, to an iron bar, which ſtood in a glaſs veſſel, which was filled with water and filings of braſs; and that the profeſſor ſtood ſo near that iron rod, that his face was within a foot of it. Now if there was no wire, that went from that iron rod, or from any part of the wire above it, into the earth, it is no great wonder, that the profeſſor was killed. I ſhould be extremely glad to be informed, whether the iron rod upon his houſe, at the time the experiment was made,

made, had any communication, by means of metal, with the earth. For, if it had, there is then more danger attending theſe experiments than I imagined. It is likewiſe ſaid in that paper, that from the electrical needle, which he obſerved, there was no danger. I am at a loſs to know what that electrical needle was, and ſhould be glad to be informed. I know, that a magnetic needle placed on a ſharp point upon the prime conductor, as ſoon as the conductor is ſufficiently electrified, will move round with ſo great rapidity, that in the dark the electricity, thrown off from both poles of the needle, will appear like a circle of fire.

Anſwers of Dr. Lining *to the Queries ſent to him.*

Query 1. In what manner, and of what materials, was your kite, and the ſtring, by which you flew it, made? and to what height did it riſe above the earth?

Anſwer. The kite, which I uſed, was made in the common way; only, in place of paper, I covered it with a ſilk, called *alamode*. The line was a common ſmall hempen one of three ſtrands. A ſilk line, except it had been kept continually wet, would not conduct the electricity; and a wire, beſides other inconveniencies, would have been too heavy. I had not any inſtrument, whereby I could take the height of the kite; but, I believe, it was at leaſt 250 feet high. It was flown in the day-time.

Query 2. You ſay alſo, " *All* the electrical fluid, " or lightning, was drawn from the cloud, and diſ- " charged in the air; and a greater degree of ſere- " nity ſucceeded, and no more of the awful noiſe

" of thunder, before expected, was heard." Now I ſhould be glad you would inform us, whether the ſerenity in the air you mention, was ſuch, as generally follows, after the clouds in the ſummer thunder-ſtorms have diſcharged ſeveral loud thunder-claps; and whether any flaſhes of lightning appeared in the ſkies, after you had diſcharged the cloud of its lightning by the kite, as commonly do after a thunder-ſtorm is over in a ſummer's night? For if there were no appearance of ſuch flaſhes, then, I think, your aſſertion, that *all* the electric fluid, or lightning, was drawn from the cloud, ſtands fully proved; but if there were ſuch flaſhes after, I conceive there muſt have been ſome of the electrical matter left behind.

Anſwer. During the time of my drawing the lightning from the cloud, and for ſome little time afterwards, it rained; by which means, the body of the cloud being diminiſhed, a greater degree of ſerenity neceſſarily ſucceeded; and the quantity of lightning extracted from the cloud, or rather its atmoſphere, proved ſufficient to prevent any thunder in town that afternoon; though there was a great appearance of thunder before the kite was raiſed. But whether the ſame ſerenity ſucceeded, as frequently happens after a thunder-ſtorm, and whether there were any flaſhes of lightning ſeen in the evening, I cannot now recollect. If ſuch flaſhes had afterwards been ſeen in the ſkies, as is common in a ſummer's evening, eſpecially after a thunder-ſtorm, thoſe might proceed from other clouds, which had paſſed the town, at too great a diſtance to be acted upon by the kite.

If

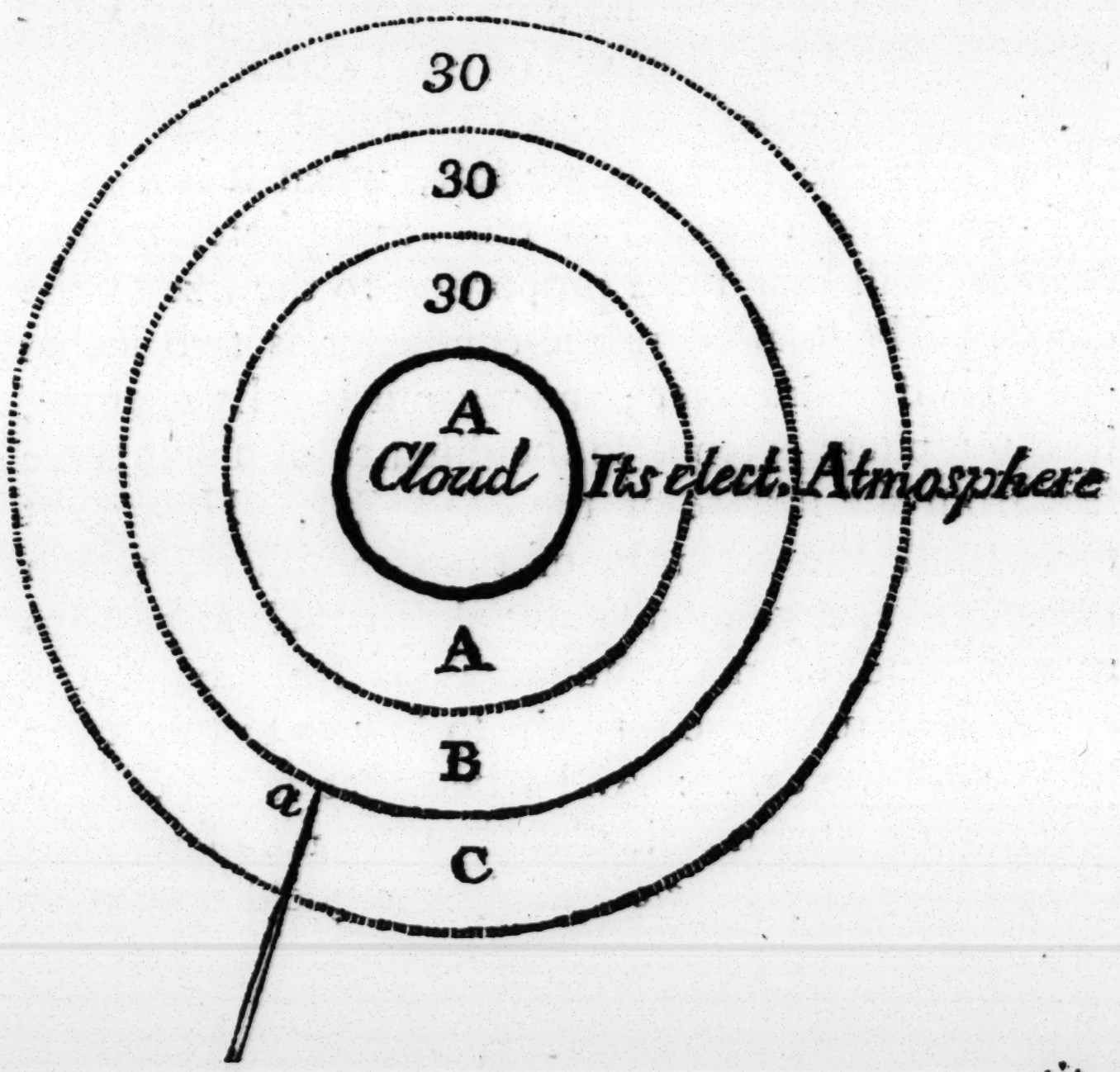
If three electrified clouds (which we ſhall call) *A*, *B*, and *C*, paſs from weſt to eaſt, and are ſo ſituated to one another, and to this town, that the cloud *B* is not only in breadth and length equal to the area of the town, but vertical over it, having an electrical atmoſphere, which reaches low enough to be acted upon by ſharp points, either raiſed by kites, or erected upon the houſes; then the cloud *B* may be ſo far deprived of its electricity, before it has paſſed over the town, as to be incapable of giving any flaſhes of lightning, till it meets with other leſs electrified clouds, and approaches very near them. But the clouds *A* and *C*, being at too great a diſtance, in their paſſage by the town, to be acted upon by thoſe points, fly on in their electrical ſtate, till they meet with clouds, or other bodies, in a non-electric ſtate, or at leaſt with thoſe, which have a leſs degree of electricity. If the cloud (ſuppoſe) *A* meets, or rather approaches afterwards near enough, the cloud *B*, which was deprived of great part of its electricity, or any other non-electric cloud, it will diſcharge at once part of its electricity into *B*, or the non-electric cloud; and if into the latter, which we ſhall call *D*, in that exploſion, it will deliver ſo much of its electricity to *D*, as will make an equilibrium in electricity between them; *i. e.* if *A* had 100 degrees of electricity, and *D* only its natural quantity, after the exploſion, each will have 50. Then *D* is in a condition to flaſh into a non-electric cloud; but that exploſion will be much weaker than that was from *A*; becauſe *D*, in exploding into a non-electric cloud, will only part with one-half (if thoſe two clouds are of equal dimenſions); that is with 25 de-

grees

grees of its electricity. But if it exploded upon a houſe, tree, or the like, the loudneſs of that explo-ſion would be equal to the firſt; as in this caſe, it has an opportunity of parting, at once, with its whole charge of electricity, which, by ſuppoſition, was 50. Hence we ſee, that one electrified cloud *only* may be productive of many claps of thunder; and that thoſe proceeding originally from that cloud, will become weaker, as the whole quantity of electricity may at laſt be divided amongſt many clouds; and from hence ſeveral phænomena, which appear during a thunder-ſtorm, and which ſucceed it, may be accounted for. If therefore any flaſhes of lightning were ſeen that evening, after the experiment with the kite, you ſee how thoſe might be produced, whether the cloud was deprived of its electricity or not.

Electrified clouds have an electrical atmoſphere, as well as the prime-conductor, when it is electrified; and the diameter of that atmoſphere, *cæteris paribus*, will bear ſome proportion to the ſize of the cloud. My ſmalleſt prime-conductor is two inches and an half in diameter; and when it is fully charged, its atmoſphere extends to the diſtance of about three feet from the ſurface of the conductor. How great then muſt the extent be of the atmoſphere, which ſurrounds a large cloud fully electrified? It perhaps may extend to many hundreds of feet round the cloud, and may even reach ſo low as to touch the ſurface of the earth: And when that is the caſe, a man, or a rod of metal, placed on a cake of reſin on the ground, may be electrified, and yield ſparks of fire.

When a ſharp point is preſented to that atmoſphere, it cannot deprive the cloud of its whole quantity of electricity, except the ſharp point be ſo near, that the cloud may explode upon it; and, in that caſe, the cloud muſt have a communication with the ground, by means of ſome non-electric body. Suppoſe an electrified cloud to have an atmoſphere, which extends round it to the diſtance of 90 feet from its ſurface; and let that atmoſphere be divided into three parts *A*, *B*, and *C*, each 30 feet in diamater: Now if a ſharp metalline point (*a*) erected on a kite, or otherwiſe, is placed either vertically or horizontally in the moſt interior part of the atmoſphere *C*, that point will continue to act till a quan-



tity

tity of the lightning is drawn off, equal to the quantity which was contained in that atmoſphere, and no longer. For then the ſemi-diameter of the atmoſphere being reduced to 60 feet, every part of it is above, and not in contact with, the ſharp point *(a)*, and conſequently beyond its ſphere of action. But let the ſharp point be then advanced into the atmoſphere *B*, and it will act as before, *&c.*

The truth of this, however contradictory it may be, to the general opinion of the action of ſharp points, in drawing off the electricity or lightning *, may be illuſtrated by the following experiment on the prime-conductor. Electrify the prime-conductor in a dark room, and draw back the globe to a ſufficient diſtance from the prime-conductor, to prevent its being ſupplied with any more electricity from the globe, while you are taking off the electrical atmoſphere with a ſharp point. Bring then a ſharp point, either vertically or horizontally, or in any other direction, within two feet of the prime-conductor; and the point, for ſome time, will appear luminous. After that light diſappears, advance the point three or four inches nearer to the conductor, and you will obſerve the ſame phænomena as before; and by advancing the point gradually in this manner, as the light upon it diſappears, the point will be alternately luminous and dark, till you have taken off the whole atmoſphere in different laminæ. As the

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* Mr. Franklin ſays, ſpeaking of ſharp points, "At whatever "diſtance you ſee the light, you may draw off the electrical "fire." page ii.

point appears more and more luminous, the nearer that it approaches the prime-conductor, the electrical atmoſphere may have different degrees of denſity, being perhaps denſer near the prime-conductor, and rarer at a greater diſtance from it. If a phial is charged on the prime-conductor, when this experiment is made, the light upon the ſharp point will be much greater, and continue longer.

Query 3. Did you make any trial, at what diſtance you could kill an animal with a diſcharge of the electrical fluid from the key or the bottle ſuſpended to it?

Anſwer. I have not hitherto had an opportunity of making any ſuch experiments with the kite. But as to the firſt, I apprehend, that no animal could be killed by the diſcharge of any quantity of electricity accumulated on the key; as the key in that experiment anſwers the ſame end as the prime-conductor, and, like it, is capable of receiving *only* a certain charge of electricity, except the lightning flows down the line too faſt, or the kite be ſo near the cloud that it may explode, when one ſtanding on the ground approaches the key to draw ſparks from it: but ſuch an exploſion would probably be fatal to the operator.

When a phial is ſuſpended to the key, after it has received its charge, if you let it continue hanging on the key, the ſurcharge will fly off from the hook of the phial, and the phial, when charged in that manner, will not give a greater ſhock than if it had been charged in the common way with the globe.

J. L.

LXXXIX.

LXXXIX. *An Answer to Dr.* Lining's *Query relating to the Death of Professor* Richman. *By Mr.* William Watson, *F.R.S.*

Read July 4, 1754. DR. Lining's letter of the 14th of January 1754, being communicated to the Royal Society by Charles Pinckney, Esq; that learned body referred it to Mr. Watson, one of their members, in order that the best information, that could be procured upon this subject, should be transmitted to Dr. Lining, for whose correspondence the Society had for many years had a very particular attention. Mr. Watson imagined, that it would be agreeable to Dr. Lining, as his abode is so remote from Petersburg, where the accident happened, to have transmitted to him not only the answer to what he more particularly requests, but also as general an account of every thing relating to so uncommon an accident, as could be procured.

The description of Professor Richman's apparatus, hereafter mentioned, was sent by himself to Professor Heinsius of Leipsic. He intituled it an electrical gnomon. To the construction of this gnomon were necessary a rod of metal, a glass jar, a linen thread of a foot and half in length, to one end of which was fastened half a grain of lead, and a quadrant *. The rod of metal *C D* was placed in the glass vessel *E*, which contained filings of metal. The linen thread *C G* was fastened to the rod at *C*, and, when the apparatus is not electrised, hangs per-

* See the figure annexed, which, with the account of the apparatus, is extracted and translated from a Latin treatise of Professor Winkler, transmitted to the Royal Society from Leipsic.

pendicular

pendicular thereto. The radius of the quadrant, which was divided into degrees, was two lines more than a foot and half in length. And here muſt be added an account of the other part of the apparatus, which was to communicate the electricity to the gnomon during a thunder-ſtorm. Through a glaſs bottle, the bottom of which was perforated, paſſed an iron rod, which was kept in its place, by means of a cork fitted to the mouth of this bottle, through which cork likewiſe was inſerted the iron rod. A tile was removed from the top of the houſe; and upon this opening was placed the bottle, ſupported by the neighbouring tiles, in ſuch manner that one end of the iron rod was not only four or five feet above the top of the houſe; but the other end, which came through the bottom of the bottle, did no-where touch the tiles, or any other part of the houſe. To this end of the iron rod was faſtened an iron chain, which was conducted into the chamber of Profeſſor Richman, upon electrics *per ſe*, ſo as no-where to touch the building. The entrance to this chamber faced the north; and at the ſouth end of it there was a window, near which ſtood a table four feet in height. Upon this the Profeſſor placed his electrical gnomon, and connected it with the chain, which was brought under the ceiling of the room over this table, and communicated with the apparatus on the top of the houſe, by means of a wire *B C*, which hung from the chain, and was joined there to *A B*, by the little ring *B*, and communicated with the rod *D C* at *C*. When the iron rod at the top of the houſe was affected by the thunder, or otherwiſe ſuitable condition of the atmoſphere, the thread beforementioned deviated from the perpendicular; as it

would alſo do, if artificially electriſed. The Profeſſor always obſerved a greater aſcent of the thread from artificial electricity than from that from the atmoſphere. By the former, he had ſeen it upon the quadrant deſcribe an angle of above fifty-five degrees, but never above thirty by the latter. In the year 1752, Aug. 9. the apparatus acquired ſo great a degree of electricity from the atmoſphere, that from the end of the rod the electrical flaſhes might be heard at ſeveral feet diſtance. Under theſe circumſtances, if any one touched the apparatus, they felt a ſharp ſtroke in their hand and arm.

Profeſſor Richman ſometimes added to this apparatus a glaſs bottle of water, after the manner of Profeſſor Muſchenbroek *H I*, adapted to a veſſel of metal *I K*, which was placed upon glaſs. The wire from the mouth of the bottle of water *H L*, during the time of the thunder, he cauſed to communicate with *B C*. From this addition he found the electricity from the atmoſphere more vehement than it was without it. This he firſt obſerved on May 31, 1753, when the electrical fire exploded with ſuch a force, that it might be heard at the diſtance of three rooms from the apparatus. On the left hand of the bottle was placed a ſecond electrical gnomon *. When this was made uſe of, the wire of metal *B C*, and the wire *H L*, were connected with *M B L*, a prime-conductor from an apparatus for artificial electricity; *viz.* a glaſs globe, *&c.* At the ſame time alſo, from the chain *A B* was faſtened a piece of wire *B K*, which was in contact with the veſſel *I K*. By theſe means, when the electrical machine was put in

* See the figure.

motion,

motion, both the electrical gnomons were electriſed; but this went off in a great meaſure, as ſoon as the motion of the machine ceaſed. By this whole apparatus taken together, Profeſſor Richman obſerved a kind of reciprocation in the effects of electricity: for at firſt, when the electrical machine was put in motion, both the linen threads *C G* and *C G* aroſe with the degrees of their reſpective quadrants. If then the wire *B C* of the right gnomon was touched, the thread *C G* collapſed to the rod *C D*; but the thread on the left ſide continued diverging as before the touch. Then, if the wire *B C* of the left gnomon was touched, then in its turn the thread *C G* at the rod *C D* of the right gnomon collapſed, and the thread of the right gnomon aſcended again. This reciprocation of the aſcending and deſcending of the thread, might be repeated three or four times without exciting the machine anew.

The ingenious and induſtrious Profeſſor Richman loſt his life on the ſixth of Auguſt 1753, as he was obſerving, with Mr. Sokolow, engraver to the Royal Academy at Peterſburg, the effects of electricity upon his gnomon, during a thunder-ſtorm. As ſoon as his death was publicly known, it was imagined that the lightning was more particularly directed into his room by the means of his before-mentioned apparatus. Nor, when this affair was more inquired into, did this opinion appear to be ill-founded: for Mr. Sokolow ſaw that a globe of blue fire, as big as his fiſt, jumped from the rod of the right gnomon *C D*, towards the forehead of Profeſſor Richman, who at that inſtant was, at about a foot's diſtance from the rod, obſerving the electrical index. This

globe

globe of fire, which ſtruck Profeſſor Richman, was attended with a report as loud as that of a piſtol. The metal wire *B C* was broke in pieces; and its fragments, thrown upon Mr. Sokolow's cloaths, did from their heat burn marks of their dimenſions thereupon. Half of the glaſs veſſel *E* was broken off, and the filings of metal therein were thrown about the room. From hence it is plain, that the force of the lightning was collected upon the right rod *C D*, which touched the filings of metal in the glaſs veſſel *E*. Upon examining the effects of lightning in the Profeſſor's chamber, they found the door-caſe ſplit half-through, and the door torn off, and thrown into the chamber. The lightning therefore ſeems to have continued its courſe along the chain, conducted under the ceiling of the room; but that it came from the apparatus at the top of the houſe to the door, and then into the chamber, does not, as far as can be collected, appear.

If indeed it could be aſcertained, that the lightning, which was the death of Profeſſor Richman, was collected upon the apparatus, for this reaſon, becauſe theſe bodies, at the inſtant of the lightning, were capable of attracting and retaining the electricity, it would then be in our power ſometimes to divert the effects of lightning. But of this fact, more time and longer experience muſt acquaint us with the truth.

From hence Mr. Pinckney may acquaint Dr. Lining, that, in Mr. Watſon's opinion, at the time Profeſſor Richman was killed, his apparatus was perfectly inſulated, and had no communication with the earth, by the means of metallic or other ſubſtances, readily conducting electricity; and that the

great quantity of electricity, with which, from the vaſtneſs of the cauſe, the apparatus was replete, diſcharged itſelf through the Profeſſor's body, being the neareſt non-electric ſubſtance in contact with the floor, and was unfortunately the cauſe of his death. This, it is preſumed, would not have happened, had the chain, or any other part of the apparatus, touched the floor, whereby the electricity would have been readily communicated to the earth.

Since the reading of the above to the Royal Society, a treatiſe in Latin, intituled, *Oratio de Meteoris vi electrica ortis*, by Mr. Lomonoſow, of the Royal Academy of Sciences at Peterſburg, has been tranſmitted to the Society. By this, among many other curious facts, we have been informed of certain particulars in relation to the death of Profeſſor Richman; of which the following may not be improper to be inſerted here.

Mr. Lomonoſow obſerves, that, with regard to the ſudden death of the gentleman before-mentioned, the accounts, communicated to the public, contained ſome circumſtances not fairly ſtated, and others of ſome importance were entirely omitted. With regard to the firſt, it is inconteſtably true, that the window, in the room where Profeſſor Richman was *(a)*, had continued ſhut, that the wind might have no effect upon his electrometer: but that the window in the next room * *(e f d g)* was open, and the door *(d)*, which was between theſe two rooms, was half open; ſo that the draught of air might

* See Tab. XXXI. Fig. 2.

Fig. 3. p. 772.

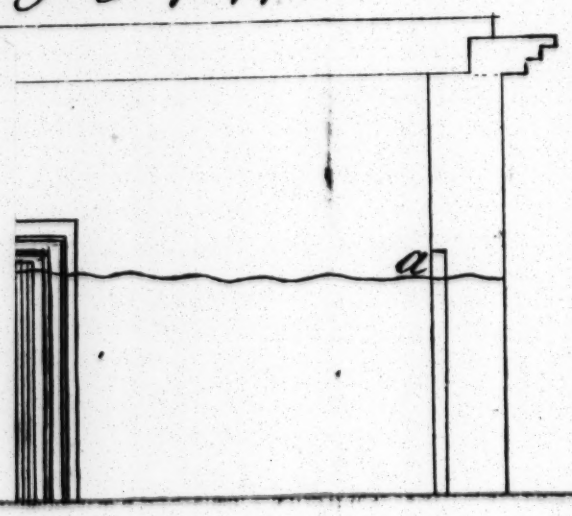


Fig. 2. p. 770.

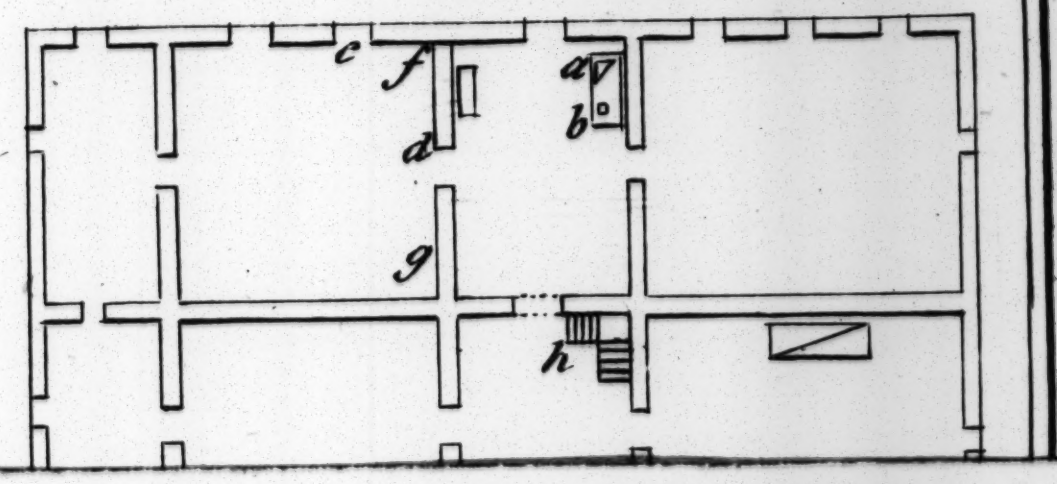


Fig. 1. p. 768.

Fig. 2. p. 770.

Fig. 3. p. 772.

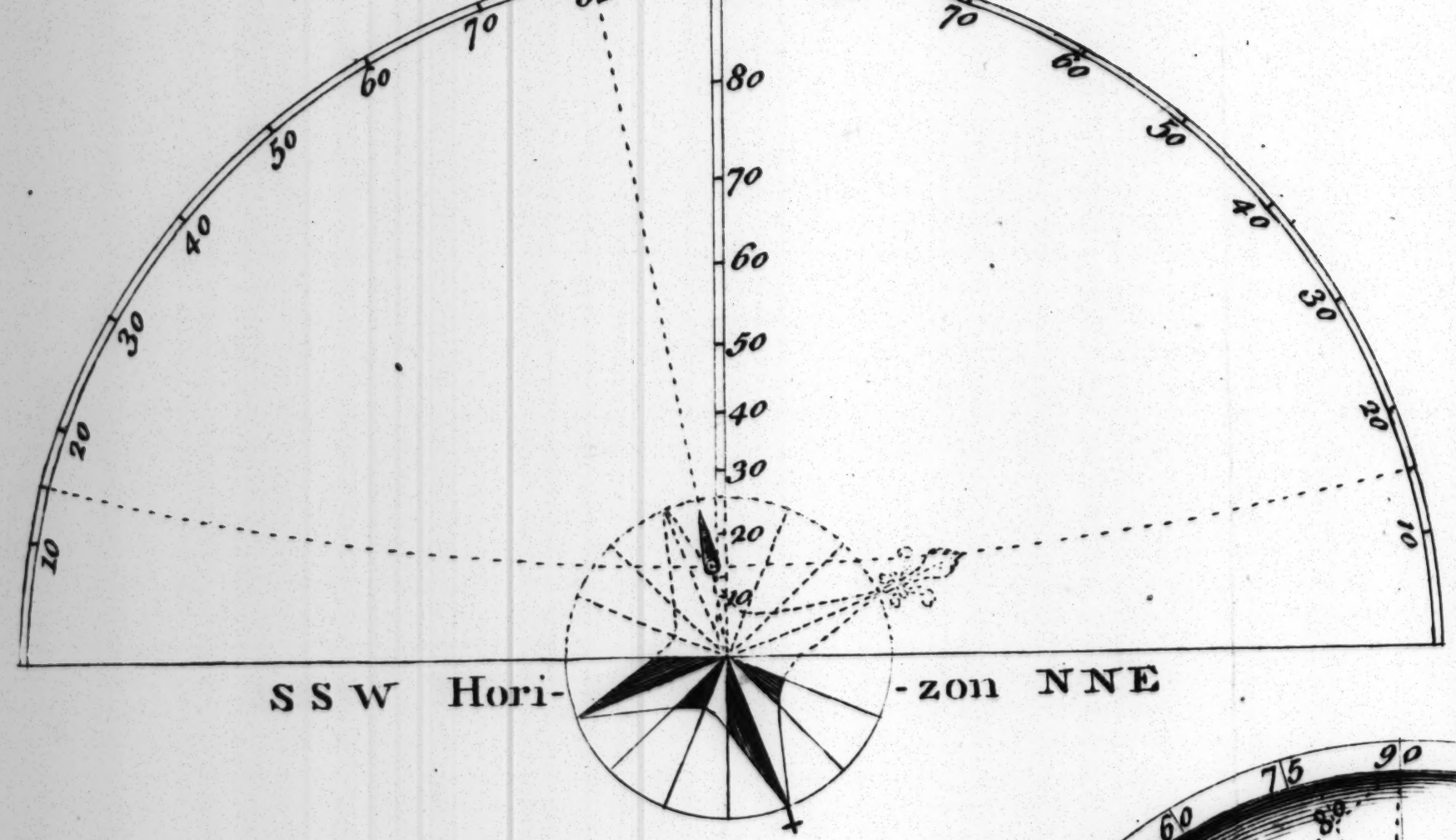
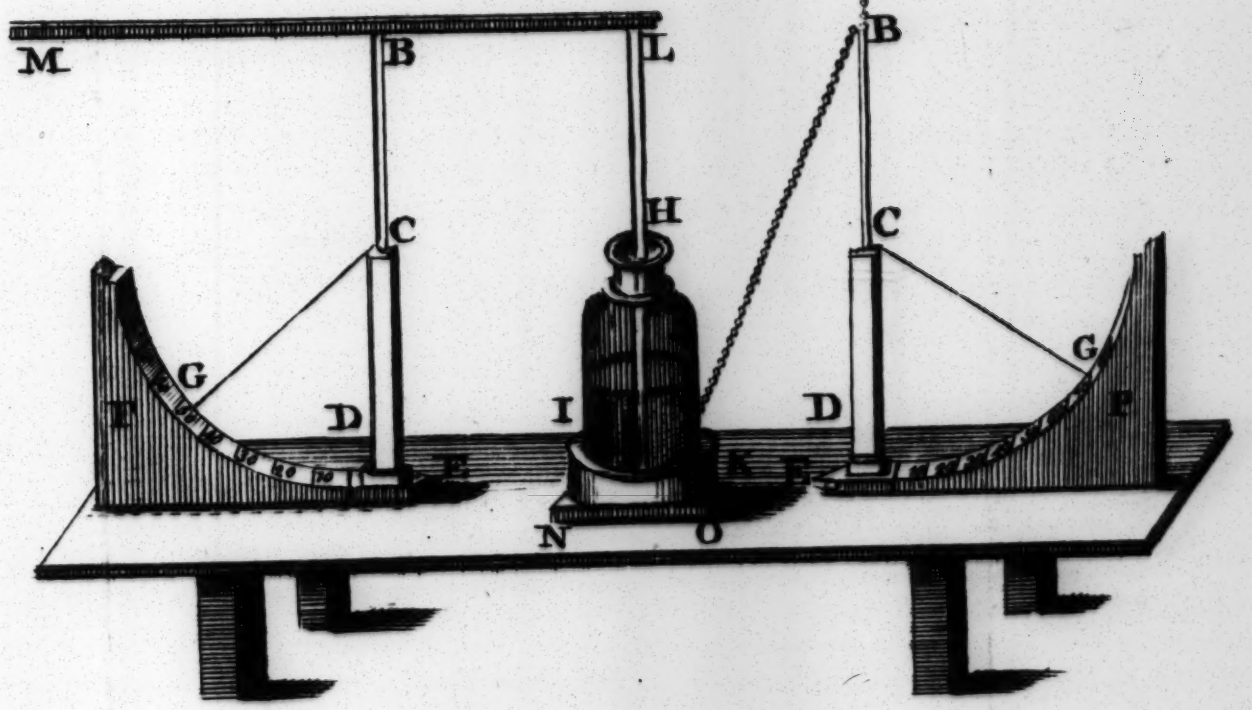


Fig. 1. p. 768.



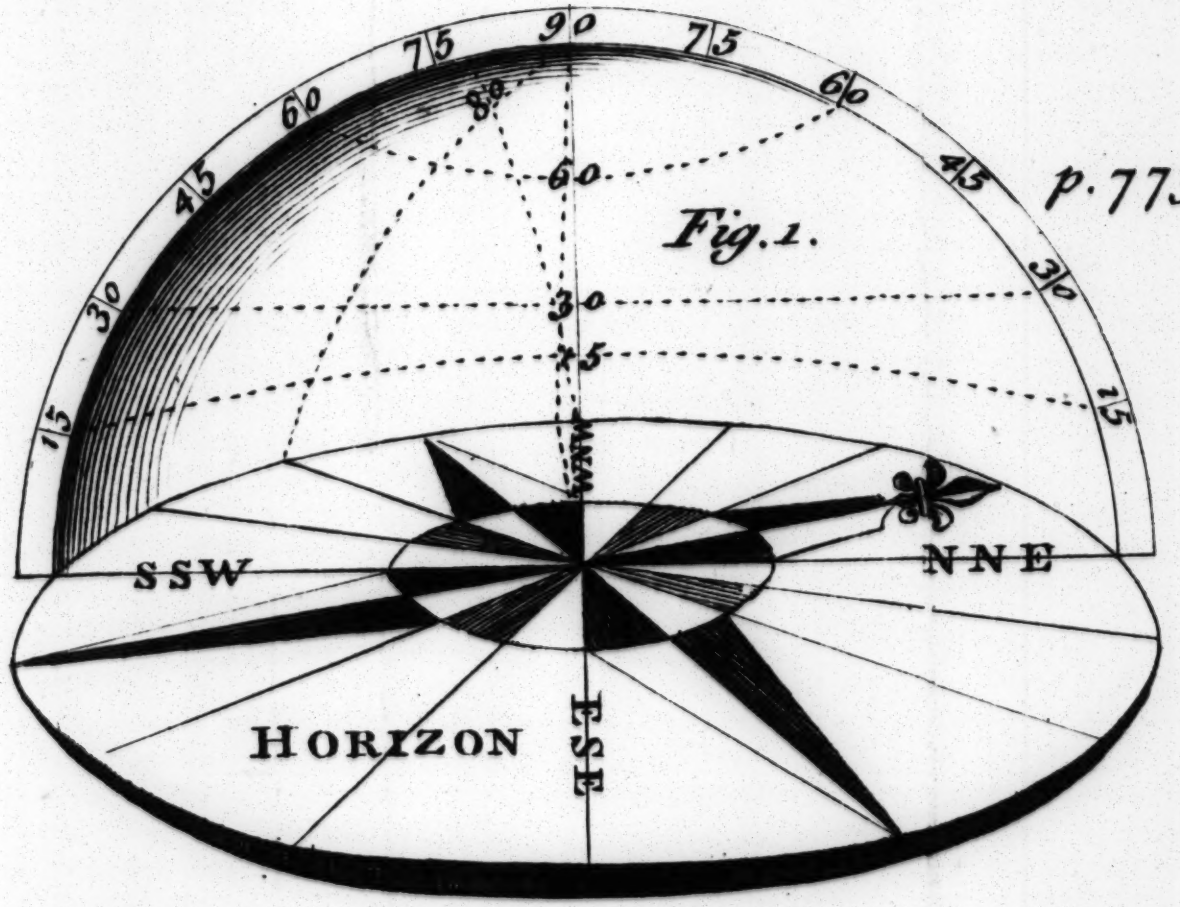
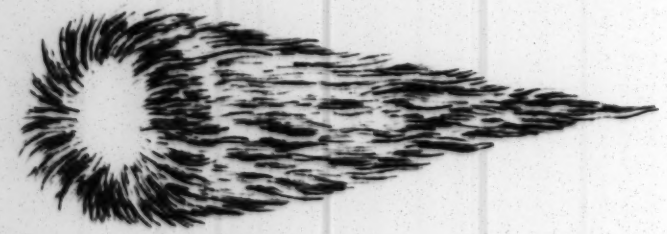


Fig. 1. p. 775.

Fig. 3. p. 775.



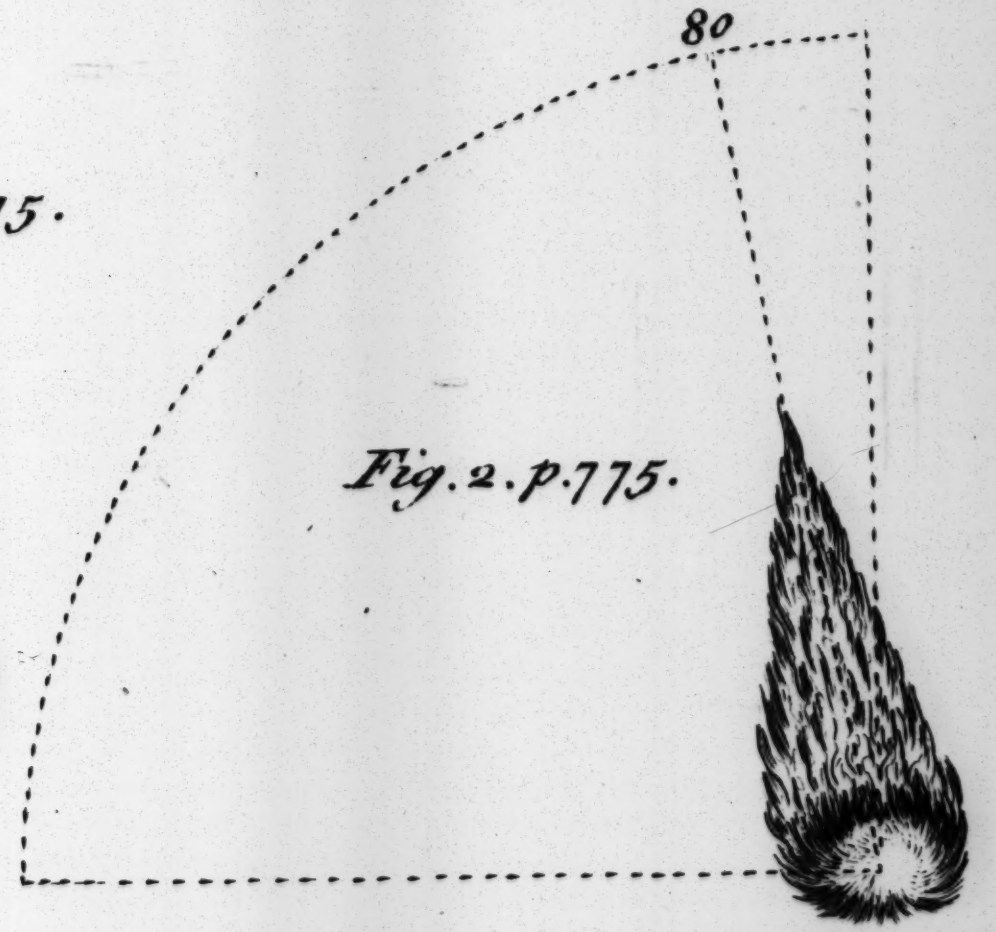


Fig. 2. p. 775.

J. Mynde sculp.

uſtly be ſuſpected to have followed the direction of the iron conductor of the Profeſſor's apparatus: that this conductor came from the top of the houſe at (*i*), and was continued to (*b*) and (*b*). 2dly, That this conductor was not placed far from that door-caſe, part of which was torn off. 3dly, That at this time no uſe was made of the Leyden bottle, mentioned in the preceding account; but the iron was inſerted into a glaſs ſtand, to prevent the diſſipation of the electrical power, and that the gnomon ſhould ſhew its real ſtrength.

With regard to the ſecond, there has as yet been no mention, that Profeſſor Richman, at the time of his death, had ſeventy rubles (a ſilver coin) in his left coat-pocket, which by this accident were not in the leaſt altered. 2dly, That his clock, which ſtood at *(f)*, in the corner of the next room, between the open window and the door, was ſtopped; and that the aſhes from the hearth *(g)* were thrown about the room. 3dly, That many perſons without-doors declared their having actually ſeen the lightning ſhoot from the cloud to the Profeſſor's apparatus at the top of his houſe. A view is likewiſe added * of the chamber, where the Profeſſor was ſtruck by the lightning; who ſtood at *(h)*, with his head projecting towards *(g)* his electrometer; at (*m*) ſtood Mr. Sokolow the engraver; from the door *(c)* a piece was torn off, and carried to (*d*): *(a b)* part of the door-caſe rent.

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* See Tab. XXXI. Fig. 3.

In this treatiſe Mr. Lomonoſow, among other phænomena of electricity, takes notice, that he once ſaw, in a ſtorm of thunder and lightning, bruſhes of electrical fire with a hiſſing noiſe, communicate between the iron rod of his apparatus and the ſide of his window; and that theſe were three feet in length, and a foot in breadth. Effects like theſe no one but himſelf has had the opportunity of obſerving.

XC. *Extract of a Letter from* John Henry Winkler, *Profeſſor of Natural Philoſophy at* Leipſic, *and Fellow of the Royal Society, to* Thomas Birch, *D. D. Secretary of the Royal Society, relating to Two electrical Experiments; tranſlated from the* Latin.

Leipſic, May 22, 1754.

Read July 4, 1754.

I Tranſmit to you, Sir, the account of two electrical experiments, both which I made the preſent year. They are founded upon the famous experiment of Leyden.

On January 8, I ſprinkled a plate of metal with the ſeeds of * club-moſs. To this plate I connected a chain, which communicated with the coating of the Leyden bottle of water. I afterwards ſufficiently electriſed this water to make the artificial thunder,

---

* *Lycopodium*, Club-moſs, Wolf's-claw.

of

of which I gave an account in my treatiſe *de avertendi fulminis artificio*, page 10 and 11. Having drawn theſe ſeeds together on a heap upon the plate, I brought over them the ſphere of metal, the ſize of which is arbitrary, impregnated with this electricity. Upon bringing this ſphere near the plate, the electricity exploded, by which the ſeeds were ſet all on fire. Theſe ſeeds were dry, and had no inflammable ſpirit mixed with them. The flame which aroſe from theſe ſeeds is true fire, as it lighted ſome flax, which lay upon the ſeeds, and extended itſelf beyond the metal.

On the 13th of January I put ſome *aurum fulminans* upon a circular piece of parchment: this parchment I cemented to a plate of metal, and cauſed the bottle replete with electricity to be diſcharged thereupon. Immediately the *aurum fulminans* exploded with a very loud report, and the circle of parchment was torn all to pieces.

XCI. *An Account of a Fire-Ball, ſeen at* Hornſey, *by* William Hirſt, *F. R. S. communicated in a Letter to* Samuel Mead, *Eſq; F. R. S.*

SIR, Hornſey, April 6, 1754.

Read May 30, 1754. HAD not illneſs prevented, I ſhould have troubled you ſooner with an account of a phænomenon, which I happened to ſee on the 26th of February laſt, about five minutes before eleven at night.

I was

I was then going down the hill adjoining to the ſouth ſide of Hornſey-church, and was not a little ſurpriſed to find myſelf ſuddenly ſurrounded with a light equal to that of the full moon, though the moon (which was then four days old) had been ſet about fifty minutes.

I ſhould perhaps have ſeen this appearance ſooner than I did, if I had not had a lanthorn with me. The diſadvantage however, which this might be, was more than balanced by the opportunity it gave of looking upon my watch, which a day or two before was adjuſted by a meridian line and an equation table.

As ſoon as I perceived the ſky ſo ſtrongly illuminated, I turned towards that part of the horizon, from whence, by the ſhadows of objects, I concluded the light muſt proceed, and had a diſtinct, though ſhort, view of a ball of fire, which, when I firſt ſaw it, appeared to be about 15° high, W. by N. Its deſcent was not exactly perpendicular, but made an angle of about 80° with the S. S. W. part of the horizon, moving from left to right, ſo that when it went below the horizon, its bearing from me was W. N. W.

It moved with great velocity, not continuing viſible much longer than two ſeconds; though (the air being clear and favourable) I did not loſe ſight of it till it deſcended below the horizon. But ſhort as this duration was, its ſhape might be well diſcerned. The diameter of the nucleus or head of the meteor appeared to be equal to the ſemi-diameter of the meridional full moon, and the tail, which terminated in a point, ſeemed not longer than twice the diameter of the nucleus. *See* TAB. XXXI. where

Fig.

Fig. 1. Shews its ſituation with regard to the cardinal points of the compaſs.

Fig. 2. Its declination from the zenith, and its altitude, when I firſt ſaw it.

Fig. 3. Is a draught of its form, and of the proportion of the nucleus to the tail.

This meteor was not attended with any noiſe, nor left any luminous ſtream after its deſcent below the horizon; neither have I heard of its exploſion.

The appearance of ſuch meteors at that cold ſeaſon of the year is the more extraordinary, as their generation is attributed to exhalations cauſed by heat, or the action of the ſun; for which reaſon they are generally ſeen after hot ſultry weather.

By the diſtinctneſs and red fiery colour of this phænomenon, I imagined, that it was not very high in the atmoſphere; but ſhould be induced to think otherwiſe, if credit is given to the following extract from the public papers. Gentleman's Magazine for March 1754. p. 141.

" Dublin, February 26. Between ten and eleven " at night a meteor appeared in the ſky, which aroſe " in the weſt, and reſembled two human bodies, " which ſeemed to run at each other with great ra- " pidity; illuminated the whole hemiſphere, and " continued about four ſeconds."

This account ſeems to be ſo much blended with ſuperſtition, that poſſibly the form and number of the appearance might be nothing elſe but the extravagant ſuggeſtions of a fearful imagination. The near agreement however in theſe two accounts, as

to

to the ſituation and time (allowing for the difference between the meridians of Dublin and Hornſey), it being nearly half an hour paſt ten at Dublin when eleven here, makes it very probable, that it was one and the ſame meteor; which, if ſo, is a proof, that its height in the atmoſphere muſt be very conſiderable. I am,

SIR,

Your obliged humble ſervant,

William Hirſt.

XCII. *A Letter from Monſieur* Clairaut, *Member of the Royal Academy of Sciences at* Paris, *and F. R. S. to* Thomas Birch, *D. D. Secret. R. S. containing a Compariſon between the Notions of* M. de Courtivron *and Mr.* Melvil, *concerning the Difference of Refrangibility of the Rays of Light.*

Read July 4, 1754.

AS I was peruſing the laſt volume of the Philoſophical Tranſactions, I fell upon a memoir of Mr. Melvil, the ſubject of which having been handled in a book, that I preſented ſome time ago to the Royal Society, from a friend of mine, and written upon a matter, which I had

had formerly ſtudied myſelf, I firſt examined whether any notice had been taken of the things common to both of the works, and to what degree the two authors agreed together.

The book I ſpeak of is a treatiſe of optics, compoſed by Mr. le Marquis de Courtivron, of the French Academy of Sciences. It was publiſhed in 1752, and preſented the ſame year to the Royal Society and conſequently has the anteriority of date with regard to Mr. Melvil's paper, which was read only the 8th of March of 1753. But I am far from imagining that he would have neglected citing Mr. de Courtivron, if he had had any notice of his book.

As I do not doubt but the Royal Society would have ordered ſome mention of Mr. de Courtivron upon the ſame ſubject, if an account, ſufficiently circumſtantiated, of his book had been read before that illuſtrious body, I hope a ſhort expoſition of the queſtion will not be amiſs.

Both of theſe gentlemen thought of accounting for the difference of refrangibility, by the difference of velocity in the rays of light; which, if it really agreed with the obſervations, would give a great ſimplicity to the theory of refraction, as reducing it under the ſame laws as the theory of gravity: whereas in the hypotheſis, wherein the particles of light are endowed with tendencies different from one another, one is obliged to multiply the properties of matter.

Meſſieurs de Courtivron and Melvil went ſo far the ſame way, as to examine, whether the immerſions and emerſions of Jupiter's ſatellites could not

afford the means of diſtinguiſhing the difference of velocities between the rays of ſeveral colours.

In fact, if, according to that hypotheſis, the red rays were ſwifter than the others, it poſſibly might happen, that the ſatellite would appear of a reddiſh colour in the beginning of the emerſion; I mean before the full time required for the whole tranſmiſſion of light from the ſatellite to us.

As to the examination of the number of ſeconds between the propagation of the red and violet rays, the two authors differ widely; and I cannot help aſſerting, that Mr. de Courtivron's calculations are more ſurely grounded than the other's.

Mr. Melvil ſuppoſes, that the difference of velocity between two ſorts of rays muſt be very near the difference of their ſines of refraction, where their ſines of incidence are the ſame. From whence he concludes, that, as the ſine of refraction of the red rays is about $\frac{1}{77}$ greater than the ſine of refraction of the violet ones, the velocity of the firſt rays muſt alſo exceed the velocity of the ſecond by about $\frac{1}{77}$.

He indeed gives thoſe proportions as only being nearly the ſame; for, ſays he farther, to know exactly the ratio of the velocities from the ſines of refraction, the following problem ſhould be reſolved, which he propoſes to the learned:

" If two bodies fall, in equal angles of incidence,
" on a ſpace terminated by parallel planes, in which
" any power acts perpendicularly to the planes (ac-
" cording to the hypotheſis in prop. 94. lib. 1. of
" the *Principia*), the ratio of the ſines of the emer-
" gence to the common ſine of incidence, and con-

ſequently

" ſequently to one another, being given, to deter-
" mine the proportion of their velocities at the time
" of their incidence on the firſt plane."

But as the inveſtigation of the curve deſcribed by the rays of light, in any hypotheſis of attractive power, has been publiſhed long ago (at leaſt by me in 1738), and by ſuch a method, as leads to the ſolution of Mr. Melvil's problem, I do not doubt but if he had ſeen that method, he would have reſolved the problem, which he propoſes, and perceived what a conſiderable difference there is between the proportion of the velocities, and that of the ſines of refraction.

Mr. de Courtivron, who has made uſe of my ſolution, is arrived at the following reſult:

If p denotes the ratio of the ſines of incidence to the ſine of refraction for one of the colours, and q the ſame ratio for any other, $\frac{1}{\sqrt{1-pp}}$ to $\frac{1}{\sqrt{1-qq}}$ will expreſs the ratio, which the velocity of the firſt rays bears to the velocity of the others.

Now, in order to make uſe of ſuch a theorem, if p and q are made equal to $\frac{77}{50}$ and $\frac{78}{50}$, which are the proportions between the ſines of incidence and refraction for the red and violet rays, the ratio of the velocities ſought will come out in even numbers, that of 45 to 44, which differs entirely from Mr. Melvil's.

Thus, if Mr. Short's obſervations have led him to conclude, from Mr. Melvil's principles, that the difference of refrangibility cannot be cauſed by the difference of velocities (when the motion of light is performed in the manner of a projectile), how ſurer

may not his aſſertion be after Mr. de Courtivron's calculation, ſince they give a difference of time conſiderably greater?

I am, with the greateſt regard,

Reverend Sir,

Your moſt humble

and obedient ſervant,

Clairaut.

Hampton-Court in Middleſex. June 30, 1754.

XCIII. *A Letter to the Right Honourable the Earl of* Macclesfield, *Preſident of the Royal Society, concerning ſome new electrical Experiments, by* John Canton, *M. A. and F. R. S.*

My Lord,

Read Nov. 14, 1754.

AS electricity, ſince the diſcovery of it in the clouds and atmoſphere, is become an intereſting ſubject to mankind; your lordſhip will not be diſpleaſed with any new expeperiments or obſervations, that lead to a farther acquaintance with its nature and properties.

The reſinous and vitreous electricity of Mr. Du Fay, which aroſe from his obſerving bodies of the one claſs to attract, what thoſe of the other would repel, when each were excited by attrition; received no light till the publication of the ſecond part of Mr. Franklin's experiments; wherein it appears, that the one



one kind of bodies electrify positively, and the other negatively; that excited glass throws out the electric fire, and excited sulphur drinks it in. But no reason has yet been assigned, why vitreous bodies should receive, and resinous bodies part with this fire, by rubbing them. Some persons indeed, of considerable knowlege in these matters, have supposed the expansion of glass, when heated by friction, to be the cause of its receiving more of the electric fluid than its natural share; but this supposition cannot be made with regard to bodies of the other sort, such as sulphur, sealing-wax, *&c.* which part with it when treated in the same manner. The following experiments, first made at the latter end of December 1753, and often repeated since, may perhaps cast new light on this difficult subject.

Having rubbed a glass tube with a piece of thin sheet-lead and flower of emery mixt with water, till its transparency was entirely destroyed; after making it perfectly clean and dry, I excited it with new flannel, and found it act in all respects like excited sulphur or sealing-wax. The electric fire seems to issue from the knuckle, or end of the finger, and to spread itself on the surface of this tube, in the beautiful manner represented at *A* and *B* in *Fig.* 1. *Plate* 32.

If this rough or unpolished tube, be excited by a piece of dry oiled silk (especially when rubbed over with a little chalk or whiting), it will act like a glass tube with its natural polish. And in this case, the fire appears only at the knuckle, or end of the finger; where it is very much condensed before it enters; as at *A* and *B* in *Fig.* 2.

But if the rough tube be greaſed all over with tallow from a candle, and as much as poſſible of it wiped off with a napkin, then the oiled ſilk will receive a kind of poliſh by rubbing it, and after a few ſtrokes, will make the tube act in the ſame manner as when excited at firſt by flannel.

The oiled ſilk, when covered with chalk, or whiting, will make the greaſed rough tube act again like a poliſhed one: but if the friction be continued till the rubber is become very ſmooth, the electric power will be changed to that of ſulphur, ſealing-wax, *&c.*

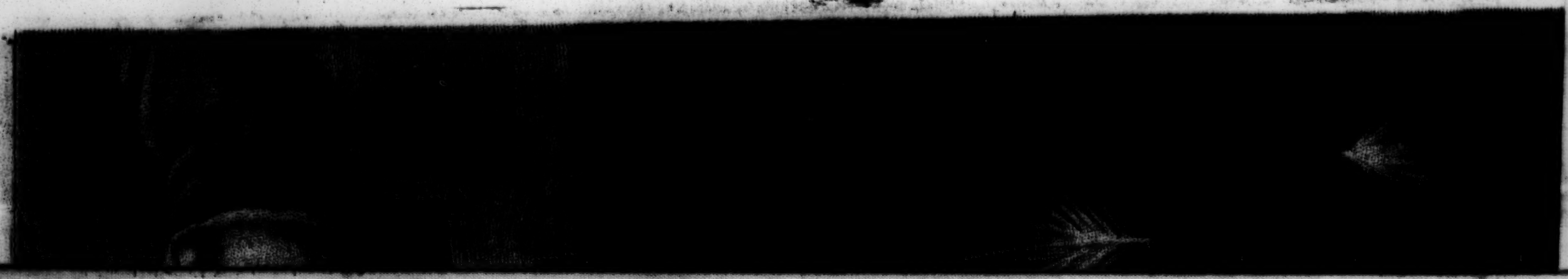
Thus may the poſitive and negative powers of electricity be produced at pleaſure, by altering the ſurfaces of the tube and rubber; according as the one or the other is moſt affected by the friction between them: For if the poliſh be taken off one-half of a tube, the different powers may be excited with the ſame rubber at a ſingle ſtroke. And the rubber is found to move much eaſier over the rough, than over the poliſhed part of it.

That poliſhed glaſs electriſes poſitively, and rough glaſs rubbed with flannel negatively, ſeems plain, from the appearance of the light between the knuckle, or end of the finger, and the reſpective tubes; but yet may be farther confirmed by obſerving, that a poliſhed glaſs tube, when excited by ſmooth oiled ſilk, if the hand be kept at leaſt three inches from the top of the rubber, will at every ſtroke appear to throw out a great number of diverging pencils of electric fire, as in *Fig.* 3. *Plate* 33; but not one was ever ſeen to accompany the rubbing of ſulphur, ſealing-wax, *&c.* nor was I ever able to make any ſenſible alteration in the air of a room, merely by the friction of

thoſe

Philos. Trans. Vol. XLVIII. TAB. XXXIII. *p.* 782 & 783.


ab1



thoſe bodies: whereas the glaſs tube, when excited ſo as to emit pencils, will, in a few minutes, electrify the air to ſuch a degree, that (after the tube is carried away), a pair of balls, about the ſize of the ſmalleſt peas, turn'd out of cork, or the pith of elder, and hung to a wire by linen threads of ſix inches long, will repel each other to the diſtance of an inch and an half, when held at arm's-length in the middle of the room. But their repulſion will decreaſe as they are moved toward the floor, wainſcot, or any of the furniture; and they will touch each other when brought within a ſmall diſtance of any conductor. Some degree of this electric power I have known to continue in the air above an hour after the rubbing of the tube, when the weather has been very dry.

The electricity from the clouds, in the open air, may be diſcovered in the ſame manner, if the balls are held at a ſufficient diſtance from buildings, trees, *&c.* as I have ſeveral times experienced, by a pair which I carry in a ſmall narrow box with a ſliding cover, (*Fig.* 4.) ſo contrived as to keep their threads ſtraight, and that they may be properly ſuſpended, when let fall out of it: and theſe balls will eaſily determine whether the electricity of the clouds or air be poſitive, by the decreaſe; or negative, by the increaſe of their repulſion, at the approach of excited amber or ſealing-wax.

To electrify the air, or moiſture contained in it, negatively; I ſupport by ſilk, between two chairs placed back to back, at the diſtance of about three feet, a tin tube with a fine ſewing-needle at one end of it; and rub ſulphur, ſealing-wax, or the rough glaſs tube, as near as I can to the other end, for three or

four

four minutes. Then will the air be found to be negatively electrical; and will continue ſo a conſiderable time after the apparatus is removed into another room.

The air without-doors I have ſometimes known to be electrical in clear weather; but never at night, except when there has appeared an aurora borealis, and then but to a ſmall degree, which I have had ſeveral opportunities of obſerving this year. How far poſitive and negative electricity in the air, with a proper quantity of moiſture between, to ſerve as a conductor, will account for this, and other meteors ſometimes ſeen in a ſerene ſky, I ſhall leave to the curious in this part of natural philoſophy to determine. That dry air at a great diſtance from the earth, if in an electric ſtate, will continue ſo till it meets with ſuch a conductor, ſeems probable from this experiment: An excited glaſs tube with its natural poliſh, being placed upright in the middle of a room, by putting one end of it in a hole made for that purpoſe in a block of wood, will generally loſe its electricity in leſs than five minutes, by attracting to it a ſufficient quantity of moiſture, to conduct the electric fluid from all parts of its ſurface to the floor. But if, immediately after it is excited, it be placed in the ſame manner before a good fire, at the diſtance of about two feet, where no moiſture will adhere to its ſurface, it will continue electrical a whole day; and how much longer I know not. It may not be improper to mention here, that if a ſolid cylinder of glaſs be ſet before the fire till quite dry, it may as eaſily be excited as a glaſs tube, and will act like one in

every

every reſpect: the firſt ſtroke will make it ſtrongly electrical.

In a paper I laid before the Royal Society, on the 6th of December laſt, I conjectured, that the electricity of the atmoſphere might be obſerved even in the winter; which I have ſince found to be true: for in the ſucceeding months of January, February, and March, my apparatus was electrified no leſs than twenty-five times, both poſitively and negatively, by ſnow, as well as by hail and rain; and to almoſt as great a degree, when Fahrenheit's thermometer was between 28 and 34, as I ever knew it in the ſummer, except in a thunder-ſtorm.

I ſhall be glad, if theſe obſervations and experiments may engage perſons of more leiſure and ſuperior abilities to purſue this inquiry; as it is highly probable their reſearches would be rewarded by many uſeful diſcoveries. I have the honour to be,

My Lord,

Your Lordſhip's moſt obedient,

Spital-Square,
Nov. 11, 1754.

and obliged humble ſervant,

John Canton.

XCIV. *Part of a Letter from* Cheney Hart, *M. D. to* William Watſon, *F. R. S. giving ſome Account of the Effects of Electricity in the County Hoſpital at* Shrewſbury.

Salop, Nov. 5, 1754.

Read Nov. 14, 1754.

WE have tried the effects of electricity in many different caſes, though I cannot ſay, that we have had much ſucceſs in its uſe, except in the caſe of one woman, whoſe left arm had been paralytic ſome years, and remained ſo, notwithſtanding all the endeavours uſed to remedy it, ſo as to be ever abſolutely motionleſs, and ſenſeleſs of heat, cold, or pain. This patient had her arm electriſed frequently, and the ſparks drawn from it, and the greateſt blows given to it for many days ſucceſſively, whereby in about eight or nine days time her arm grew ſenſible of pain and warmth, *&c.* and ſhe had ſome little motion of her fingers, being able to graſp any thing with her arm down, or before her; but ſhe could not lift it up to her head any better. This encouraged us to continue the electriſing three weeks or a month longer; in which time ſhe had got ſome little ſtrength in her arm, could open and ſhut her fingers, and lift it half-way to her head: but the pain ſhe had from the electriſing, and the fear that increaſed continually of new ſhocks, made her obſtinately to reſiſt uſing it any longer; and ſhe choſe, ſhe ſaid, rather to remain paralyaic than undergo ſuch operations any more: for which reaſons ſhe was diſcharged out of the infirmary,

with

with ſuch little relief as is above-mentioned, and I never have heard more of her. I wiſh indeed ſhe had tried it a while longer, as it bid ſo fair to do her ſervice; and this was the only caſe, which gave us any reaſonable hopes from its uſe.

Another young girl, about ſixteen, whoſe right arm was paralytic, on being electriſed the ſecond time, became univerſally paralytic, and remained ſo about a fortnight; when the increaſed palſy was removed indeed by the medicines, which her caſe indicated; but the firſt diſeaſed arm remained as before. I ſhould have mentioned too, that this arm was greatly waſted, in compariſon of the other. However, notwithſtanding the former bad accident, I had a mind to try the electricity on her again, which we renewed, and after about three or four days uſe, ſhe became the ſecond time univerſally paralytic, and even loſt her voice and tongue, and with difficulty could ſwallow: this confirmed me in my opinion, that the electric ſhocks had occaſioned theſe ſymptoms. We therefore omitted it, and the girl, though ſhe grew better of her additional palſy (for ſo I call it), remained as bad as before of her firſt; and, after about four months repeated courſe of medicines of different kinds on her, ſhe was diſcharged incurable, and ſtill remains as bad as ever.

Theſe, Sir, are the only two caſes worth noticing, that have occurred to me, wherein it can be ſaid to have produced any remarkable effects at all: for on numbers of others, that have experienced it, I have not obſerved any thing happen, except that, when the affected palſied limb is touched with the electrical conductor, a convulſive motion is produced imme-

diately; but this is over very ſoon, and they have all remained as motionleſs and bad as before. And the ſecond caſe above-mentioned has made us here very cautious in proceeding to its uſe in many patients, for fear of bringing on worſe ſymptoms than they had before, by it.

XCV. *A Letter from the Reverend* William Brakenridge, *D.D. and F.R.S. to* George Lewis Scot, *Eſq; F.R.S. concerning the Number of Inhabitants within the* London *Bills of Mortality.*

Dear Sir,

Read Nov. 21, 1754. AS I have lately had the curioſity to conſider, the number of inhabitants in London and Weſtminſter, whether they increaſe or decreaſe; I preſume to ſend you the obſervations I have made. For as no one underſtands numbers better than yourſelf, your approbation will much confirm me in my opinion, and perhaps have weight with many others; when you can eaſily find out any paralogiſm or miſtake, that I may have made. And if you can ſpare any time from your great employment, to think upon the ſubject, and to rectify any thing that may be amiſs in my method of computeing; it will be very obliging.

I have conſulted the yearly bills of mortality for the laſt fifty years, which I imagine will be ſufficient for my purpoſe; and from them I have extracted all the numbers of the baptiſms and burials, both within the

the walls of London, and at large within the bills: for I thought that within the city walls, where the number of houſes is nearly known, the baptiſms and burials might be very uſeful to reaſon upon, concerning the whole inhabitants both within and without. And becauſe it may be ſurer, to compute from a number of years taken at an average, than from the numbers in any one year as they ſtand in the bills; I have taken the ſums of the numbers, for each five years of the fifty, and then the fifth part of each of theſe ſums; which will at a medium be the number for any particular year. And in like manner, I have taken the ſums of the numbers for each ten years, and the tenth part of each of the ſums will be the number for any year, at an average. And the numbers ſo found will appear thus:

Years.	Baptiſms.	Burials.	Baptiſms.	Burials.
1704— 8	1870	2553	15867	22103
1709—13	1805	2551	15288	21701
1714—18	1890	2706	17586	24641
1719—23	1871	2719	18360	26978
1724—28	1829	2727	18442	27670
1729—33	1578	2532	17452	26267
1734—38	1406	2242	16762	26165
1739—43	1221	2397	15034	28219
1744—48	1062	1989	14402	23884
1749—53	1087	1790	14850	22006
1704—13	1837	2552	15577	21602
1714—23	1880	2712	18073	25809
1724—33	1703	2647	17920	27168
1734—43	1313	2320	15898	27192
1744—53	1074	1890	14626	22945

Where

Where the numbers are ranged in five columns. The firſt denotes the years, the ſecond and third the baptiſms and burials within the city walls, and the third and fourth ſhew the baptiſms, and burials at large within the bills. Thus, for inſtance, 22945 is the number of burials, at a medium, for any of the ten years within the bills from 1744 to 1753 incluſive. And in like manner, 1221 is the number of baptiſms for any year, at an average for five years, from 1739 to 1743 incluſive, and ſo of others. The numbers above the line are computed for five years, and thoſe below are for ten.

In the burials it is always to be conſidered, that there are perhaps 2000 more, than what the bills repreſent them. For there are burying-grounds belonging to the Proteſtant Diſſenters, the Quakers, and the Jews; of which there is no account taken, and that are very conſiderable. In the firſt of which in Bunyan-Fields, I have been informed there are about 400 burials in the year, and in the others, together, there may be about 400 more; which ſum of 800 we may ſuppoſe comes from all parts within the bills. But I think the one-half, *viz.* 400, muſt at leaſt come from within the city; where there are moſt Proteſtant Diſſenters and Jews. So that 400 may always be added to the burials, within the city. It is likewiſe to be remembered, that both from within and without the city, a great many burials go out into the country, of which there is no notice taken. But from what I have obſerved, if we were to ſuppoſe, that there are 1200 in the whole, carried out into the country, over and above the 800 mentioned above, in the burying-grounds; I ſhould imagine that to be the

outmoſt. And therefore in our calculations we ſhall ſuppoſe 2000 burials yearly, more than in the bills at large. And which, whether we are exact enough or not in the ſuppoſition, will by no means hinder us to diſcover the increaſe, or decreaſe of the people.

It is next to be obſerved, that in the bills the baptiſms are always about two-fifth parts at leaſt, leſs than the burials, with the numbers added to them above-mentioned; and that this difference within the city ſeems continually to increaſe, ſo that it is much greater now than it was ſome years ago; which appears plainly to ariſe from two cauſes; the number of Diſſenters of various denominations, and the multitude of people that live unmarried. But I think it is rather owing to the laſt: for in London and Weſtminſter the one-half of the people at leaſt live ſingle, that are above twenty-one years of age; which muſt prevent almoſt as many more births, that might be reaſonably expected. And this is not mere conjecture; for I have had ſome proof from a particular detail given me of one pariſh within the city; where the greater part of thoſe that are above that age are ſingle. In the natural ſtate of mankind it ſeems plain, that the number of births ſhould be greater than the burials, and I believe that in many pariſhes in the country they are near double. I found it ſo in the Iſle of Wight, where I lived ſome time, and had an opportunity to ſee their regiſters; for there the births were generally near double. And even in London, before the great fire in 1666, it appears, from ſome pariſh regiſters, that the baptiſms were near about equal to the burials, and never afterwards: the reaſon of which I do not underſtand, unleſs it

be

be that more people were then married, and that from that time there was a greater confluence of ſtrangers: for there certainly were more Diſſenters at that time than ever after.

It is farther to be obſerved, that in the bills from the year 1704 to the year 1728, without the city, both the number of chriſtenings and burials continually increaſed; and that from that time to 1743, they continued nearly the ſame; but that after 1743 they gradually decreaſed till this time; which plainly ſhews, that the inhabitants were increaſing till about the year 1728; and that from thence to 1743, they remained in the ſame ſtate nearly; but that afterwards, during the laſt ten years, till 1753, they were conſtantly diminiſhing. For it is evident, that the number of inhabitants muſt always be in proportion, to the number of births, and burials conſidered together. And hence it appears, that the cities of London and Weſtminſter, were in the moſt flouriſhing ſtate, with regard to numbers, from 1728 to 1743, and that they are now paſt their height, and in the ſame ſtate they were in the year 1708; and the firſt decreaſe ſeems to have been at the beginning of the laſt French war, which was in 1744. Within the city walls the number of the inhabitants do not ſeem to fluctuate, in the ſame periods of time, as without; for the moſt numerous ſtate of the city, appears to have been from the year 1718, to the year 1728, and then after that they have been continually decreaſing: ſo that when they were moſt numerous within the walls, they were not then arrived at the height without; and when they were in the higheſt ſtate without, they were diminiſhing in the city.

city. Perhaps the vaſt number of new buildings, within the liberties of Weſtminſter, may have in part cauſed this diminution. And as from the year 1718, within the city, the chriſtenings have been ſo remarkably decreaſing, that they are now but three-fifths of what they were at that time, and the burials are likewiſe diminiſhed above one-fourth in the laſt five years; this ſeems to ſhew that the inhabitants within the city walls muſt be near one-fourth fewer, than they were in the year 1718.

Now, in order to calculate the number of inhabitants, it will be neceſſary to obſerve, that in a year in London there generally dies one perſon in thirty. This Sir William Petty has long ago obſerved; and I have found it to be near the truth, upon conſulting my pariſh regiſter. For in the pariſh of Baſſiſhaw, London, there are not above 800 people, as appears from an account that I had lately given me: And the burials for the laſt ten years in the whole amount to 262; which at a medium gives 26 for one year, which is the thirtieth part of 800 nearly. In ſome pariſhes in London there die more than in this proportion, as in St. Giles's Cripplegate; and in others in the out-parts of the town there die fewer; but I believe, in general it will hold true, in and about the city. In the town of Breſlaw in Germany, from which Dr. Halley formed his famous table for the probabilities of life, there die about two in ſixty-nine, that is leſs than one in thirty-four; as is plain from an eaſy computation. But there certainly die more than in that proportion, within the London bills; for it appears, that one-third at leaſt of the children die under two years of age; whereas at Breſlaw there die

under that age, only one-fifth; and therefore the difference being two-fifteenths, or four-thirtieths, there die four in thirty more at London than at Breſlaw, under two years of age.

In the country the caſe is very different; for there does not die above one in fifty, in healthy places. Sir William Petty has likewiſe obſerved this, and I have found it true. For in the pariſh of Newchurch in the Iſle of Wight, where I reſided ſome time, there are about 900 people, and there does not die, at a medium, above eighteen yearly; which is one in fifty exactly. And I believe this will be found to be nearly the ſame, in moſt of the counties in Britain, where the people do not live in great towns; which ſhews the great difference between the effects of the air, in London and the country.

If then it be allowed, that in London and Weſtminſter there dies one in thirty, it will be very eaſy to make a calculation of the whole number of the people nearly, that are within the bills. For if we take the number of burials at an average for ſome years, and multiply that by 30, the product muſt be the number of the people. Thus if we take the number of the burials, at large within the bills, for any one of the laſt ten years, at a medium, from 1744 to 1753 inclusive, to be 22945, and add to this 2000, for thoſe burials omitted in the bills, as is ſuppoſed above, the total will be 24945, all the burials within the limits of the bills, for one year at 1753; and then multiply this by 30, the product 748350 will be the whole number of the people nearly, at preſent. But if we take 27192, the number of the burials, at a medium, for any one of the ten

ten years preceeding 1743 inclusive, and add to this 2000, as above; the whole of the burials at that time within the bills will be 29192, which being multiplied by 30 gives 875760, for the number of the people at the year 1743. And therefore the inhabitants are fewer now than they were in 1743, by 127000. I have taken the numbers, at a medium, for ten years, to avoid any uncertainty, that might arise about a computation for a single year.

If we were to try the same calculation, by taking the burials, at a medium, only for five years to 1753, and also for five years to 1743 inclusive, the difference will be greater. For the numbers at these two times will be 720180, and 906570, of which the difference is 186390; so that the people would appear fewer at 1753 than they were in 1743, by 186000. But this is not so much to be depended upon as the numbers above; because there were two extraordinary bills at 1740 and 1741. Or if we should imagine that there might not more die at London than at Breslaw, that is one in thirty-four, still the difference would be greater than we found at first. For taking the burials at an average for ten years, at 1753 and 1743, as above, the numbers would at these two times be 848130 and 992528, of which the difference is 144398; so that it seems plain, if the bills are to be depended upon, that there is a decrease of the people since 1743 of above an hundred thousand, and that at present the number is about 740000. And this decrease has been annually continued: for if we try the thing farther, at the distance of five years, and take at a medium for five years, the burials for 1753 and 1748, the numbers

will come out 720180 and 776520; of which the difference is 56340, the number decreaſed for the laſt five years.

There is another way of computing from the number of houſes; but I think this not ſo certain as the other. For here are two difficulties, to aſcertain the number of houſes, and to fix on the number of perſons for each houſe. As to the laſt, Sir William Petty thought we might allow eight perſons to a houſe; which I have found to be a miſtake. I have made an experiment of it, and got an exact account of the numbers in each houſe in a certain pariſh in London; and I find that they exactly come to ſix in a houſe, empty and full together, for there is ſeldom above one in twenty empty. And as in that pariſh the people are in a middle condition, and ſome of them have a number of ſervants; it may be preſumed they are in a middle ſtate with regard to numbers, between the very great families and thoſe in the loweſt rank. This is alſo confirmed, if we allow, as above, one in thirty to die yearly in London. For within the city walls there were 11857 houſes in the 97 pariſhes, as appears from Mr. Smart's account, which was ſuppoſed to be very accurate at that time: But ſince he publiſhed that in 1741, there are not ſo many houſes within the city walls; for in many pariſhes there are houſes greatly enlarged, ſome rebuilt in place of two or three, and warehouſes made of others. I know ſome pariſhes in which there is one in twenty fewer than in his time. In others perhaps there is no alteration. But I think they muſt, at an average, be diminiſhed three in an hundred at leaſt; and conſequently

ſequently there are about 354 fewer, and the number of houſes within the city walls is about 11503; which being multiplied by 6, gives 69018, for the number of inhabitants; which is nearly equal to the burials 2290 multiplied by 30, or 68700; taking the burials at a medium for ten years, and adding 400 as above.

The number of houſes within the bills may then be nearly come at, from the number of burials. For if we take the number of burials, for the laſt ten years, at an average, within the city to be 1890, and add 400, which makes 2290, we may ſay, if 2290 comes from 11503 houſes, then the whole number 24945 of burials within the bills, having allowed 2000 as above, muſt come from 125302 houſes. And there cannot be fewer; for there are more burials within the city, in proportion to the baptiſms, than in the out-pariſhes; and therefore more burials in proportion to the number of houſes; which ſhews that the number of houſes cannot be leſs than 125302; which being multiplied by 6, will give 751812, for the number of people for this preſent time; and it is nearly equal to the number 748350 found above. So that the numbers produced from theſe two methods being almoſt equal, this is ſome farther proof that our ſuppoſition of ſix perſons to a houſe, empty and full, is near to the truth. But if we ſuppoſe, that the number of houſes within the walls is now the ſame, as in Mr. Smart's time, 11857; then all the houſes within the bills will be 129158, and the number of people 774948 greater than 748350, found above, by 26598; which is not much in ſuch calculations.

Sir

Sir William Petty likewiſe ſays, that he was informed there were 84000 houſes tenanted within the bills, in the year 1682, in which he wrote; and if ſo, the number of houſes ſeem to be increaſed, near one-third ſince that time. And, according to our way of computing, to ſuppoſe ſix to a houſe, empty and full, there could not be more than 504000 people at that time; which is leſs than the number we found above for the preſent time, 748350, by 244350. But now, inſtead of increaſing, we are decreaſing; for ſince the year 1743 the inhabitants have been annually diminiſhed; by which it appears that this great city is paſt its height, and is rather upon the decline with regard to numbers. And hence we ſee how far Sir William was miſtaken, who imagined that it might increaſe continually till the year 1800; when the number of people would be five millions, that is near ſeven times as much as they are at preſent.

Now, to account for this decreaſe there may be various conjectures: I think three cauſes may be aſſigned, that may all operate jointly. One may be the vicious cuſtom that has prevailed of late years, among the lower people, of drinking ſpirituous liquors; another the faſhionable humour of living ſingle, that daily increaſes; and a third may be the great increaſe of trade in the northern parts of Britain, that keeps the people there employed at home, that they have no occaſion, as formerly, to come hither for buſineſs; and it were to be wiſhed that this cauſe was the moſt prevailing. But whatever be the cauſe, it ſeems plain to me, that it could not be the late French war, as ſome imagine. For by what

was

was ſhewn above, there has been a decreaſe of 56000 ſince the year 1749, after the peace; but if the war had been the cauſe, there ought rather to have been an increaſe after it. And as in the whole, we could not have loſt more than 150000, in the war by land and ſea, of which there was not one-fifth, or 30000, taken from about the city; this can never account for 64000, the decreaſe before the year 1748. In the former war, between 1702 and 1711, the city never decreaſed, but continually increaſed: from which one would imagine that the laſt war could not diminiſh its numbers.

Nor can this decreaſe in the bills be accounted for, from a greater number than formerly leaving the town in ſummer; becauſe it does not appear that there is a greater number of ſuch, than was ten years ago. And if it could be allowed that the number was greater, it can never be thought that it can amount to 120000 more than in year 1743.

It is true, this decreaſe may appear ſuprifing to ſome, when they ſee the number of new buildings in Weſtminſter, continually increaſing; but then, on the other hand, it is likewiſe to be conſidered, that there are a great number of houſes enlarged, or re-built, in place of two or three others, as mentioned above; and others falling in, and empty, about the eaſtern parts of the city: ſo that for the laſt twenty years the inhabitants ſeem only to be moving, from the eaſtern to the weſtern parts of the town, and not increaſing.

And now, Sir, I am afraid I have tired you with this long letter, in which I have endeavoured to repreſent the preſent circumſtances of this great city, with

with regard to numbers, and, if I have committed any miſtake, I hope your uſual goodneſs will excuſe,

Dear Sir,

Sion-College, Novem. 20, 1754.

Your moſt obedient ſervant,

Wm. Brakenridge.

XCVI. *A Letter from Mr.* William Watſon, *F. R. S. to* Charles Gray, *of* Colcheſter, *Eſq; F. R. S. in relation to a large Calculus found in a Mare.*

Dear Sir, London, July 26, 1754.

Read Nov. 21, 1754.

I Take this opportunity, by your means, of ſending back to its owner, who is ſo unwilling to part with it, the ſtone taken out of the belly of the mare, which you were ſo obliging as to ſend me. I ſhould have been glad indeed to have been informed preciſely, from what part of the abdomen of the mare it was taken; but this you was prevented from tranſmitting to me, on account of the ignorance of the perſon, who opened the mare, and who ſaid, that the ſtone was found in or near her kidneys; though I am of opinion it was formed in the inteſtinal tube. As there are at preſent no ordinary meetings of the Royal Society, I was prevented from laying this ſtone before that learned body: I ſhewed it however to ſeveral of the gentlemen, who, with myſelf, agree, that a ſtone, large as

as this, is a very great curioſity. It is compoſed of different laminæ, and its figure is that of an oblate ſpheroid, whoſe greateſt diameter is eight inches and an half; its leſſer eight inches. It ſurface is extremely regular, but appears in ſeveral of its parts, as though it had been corroded by ſome acrid menſtruum; and in a place or two, where the external lamina is quite worn away, and the lamina immediately underneath it poliſhed during its continuance in the mare, the calculus has great reſemblance in colour to occidental bezoar.

This ſtone weighed in air fifteen pounds twelve ounces avoirdupois; in water ſix pounds: ſo that its ſpecific gravity to that of water is nearly as eight to five. And you may obſerve, that it is not only conſiderably lighter than any foſſile petrifaction, but much more ſo than many animal; ſome human calculi, when freſh extracted, being to water as two to one.

With regard to its bulk, it is the largeſt I remember to have been obſerved, except one preſented to the Royal Society in the year 1737, which was taken out of the ſtomach of a dray-horſe, belonging to Sir Henry Hicks, Knt. at Deptford, and which weighed nineteen pounds avoirdupois, excluſive of the outward ſhell or cruſt, which was broken off in ſeveral pieces. Both theſe ſtones were in appearance like a pebble, and formed of different laminæ. The greateſt circumference of that you ſent me was ſomewhat more than twenty-ſix inches; that of Sir Henry Hickes's twenty-eight.

Sir Henry Hickes's horſe was twenty-two years old; and, for eleven or twelve years before he died, frequently was obſerved to be in violent pain: but

the mare, the ſubject of the preſent letter, though ſixteen years old, gave no ſigns of being in pain till about three months before her death, when ſhe would frequently lie down, and roll about. And it is the more extraordinary, that, large as the ſtone was (and it muſt have been very large for a long time) it did not diſable the mare from doing her uſual work for a more conſiderable time before her death; which did not ſeem to be occaſioned by the ſtone, ſhe dying near her foaling-time; nor ſo far diſturb her oeconomy, as to prevent her propagating her ſpecies.

The Earl of Macclesfield, our moſt worthy Preſident, Dr. Birch, and ſeveral of our friends, who have ſeen this ſtone, join with me in thanks to you for this communication.

In the year 1746, his Grace the Duke of Richmond preſented to the Society a ſtone found in the colon of a horſe, the circumference of which was ſixteen inches. His Grace at the ſame time preſented ſome other ſtones, found in the inteſtines of a mare, which were poliſhed like bezoar. It was very remarkable, that two of theſe ſtones, when ſawed aſunder, were found to have been formed each upon an iron nail, as a nucleus.

I ſhall embrace every opportunity of ſhewing you, that I am, with the greateſt reſpect,

Dear Sir,

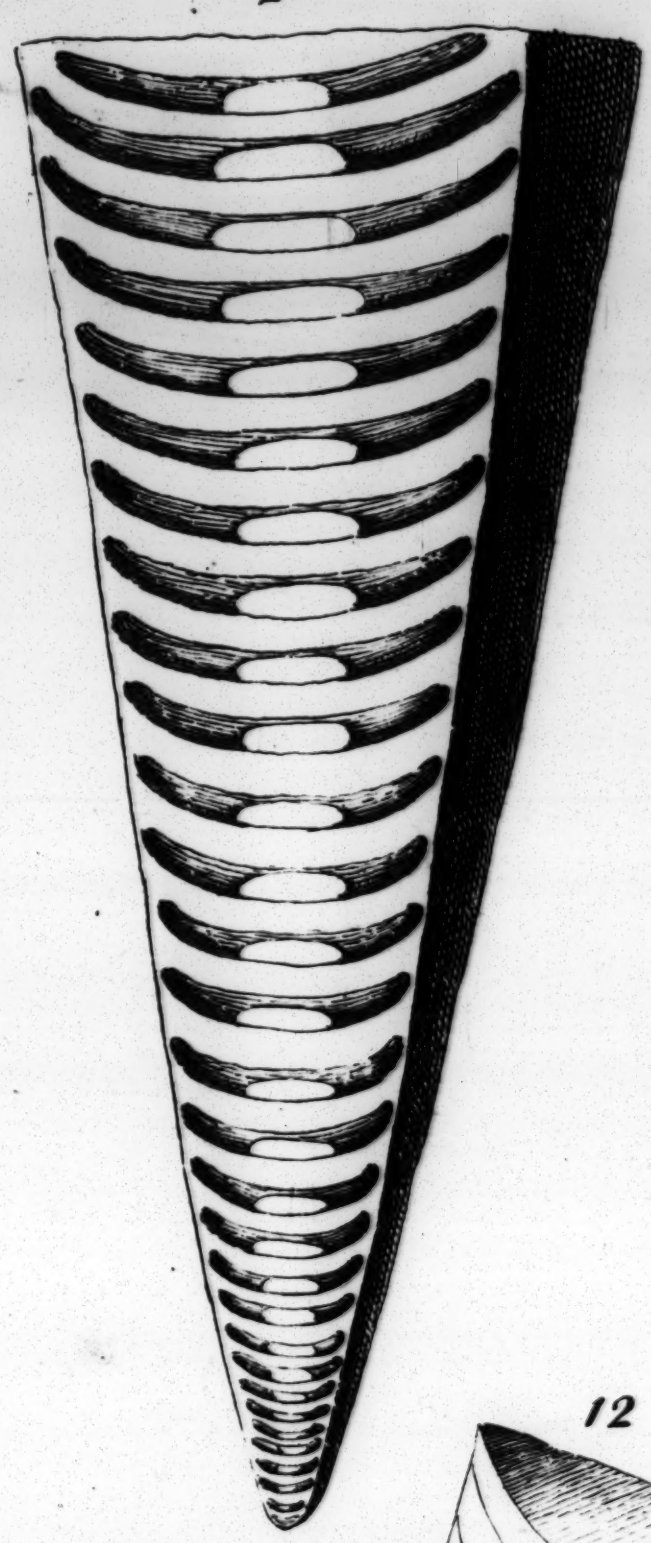
Your moſt obliged and obedient ſervant,

W. Watſon.

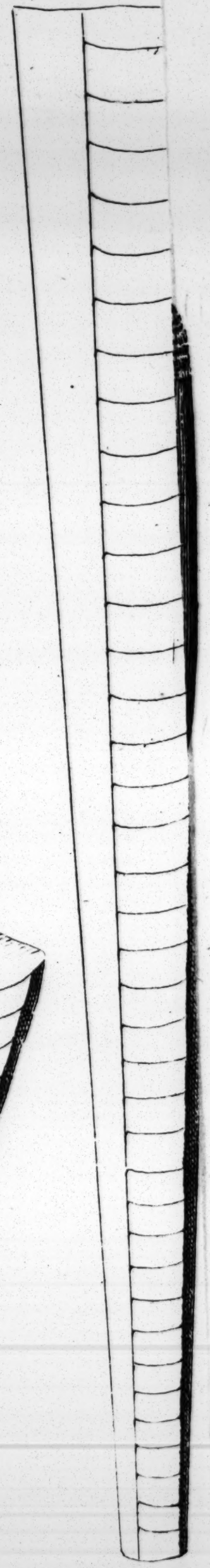
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Phil. Trans. Vol. XLVIII TAB. XXXIV. *p.*

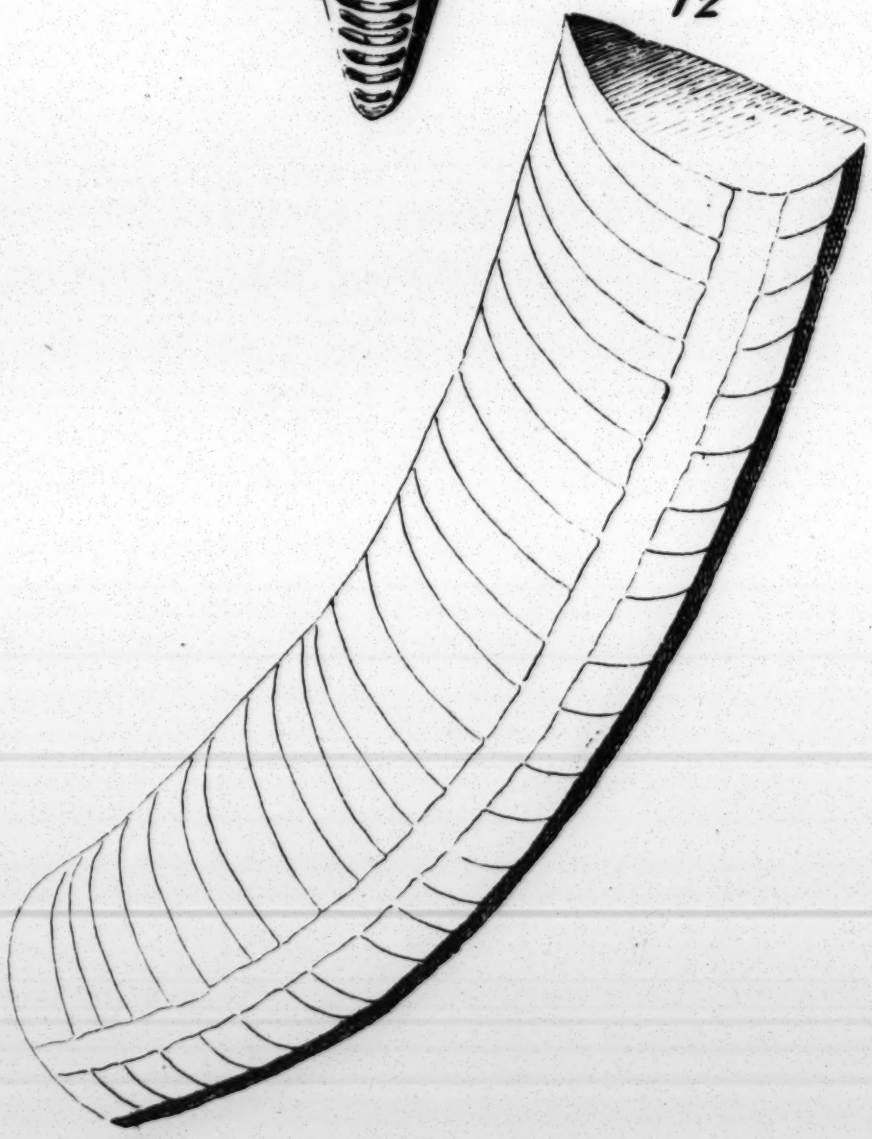
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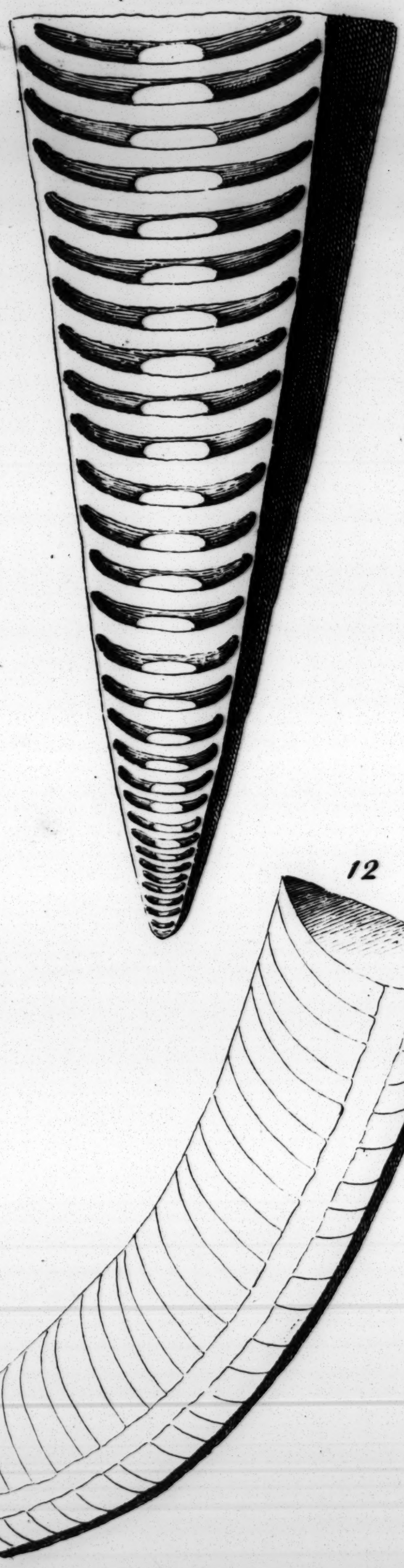


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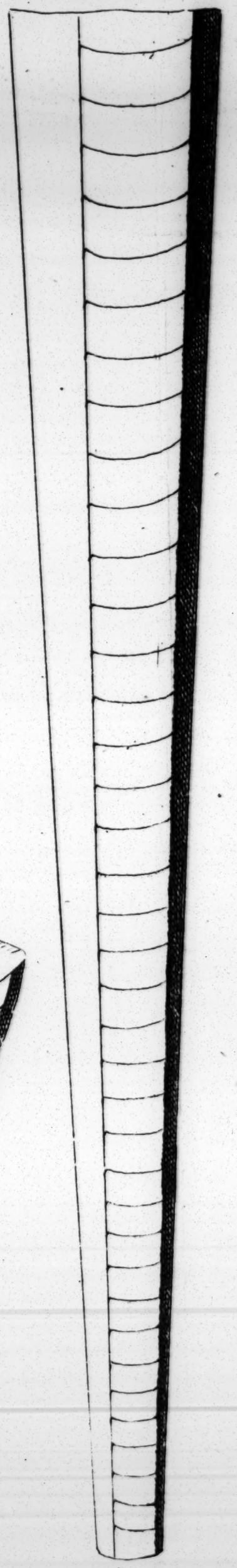


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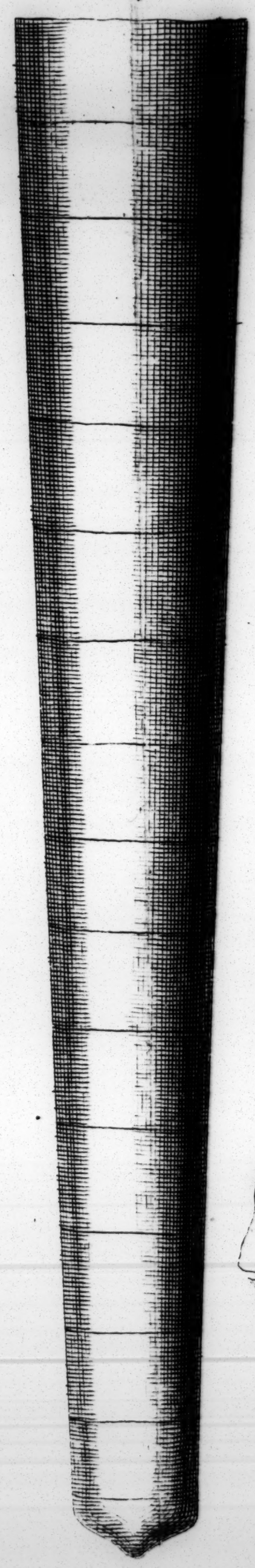
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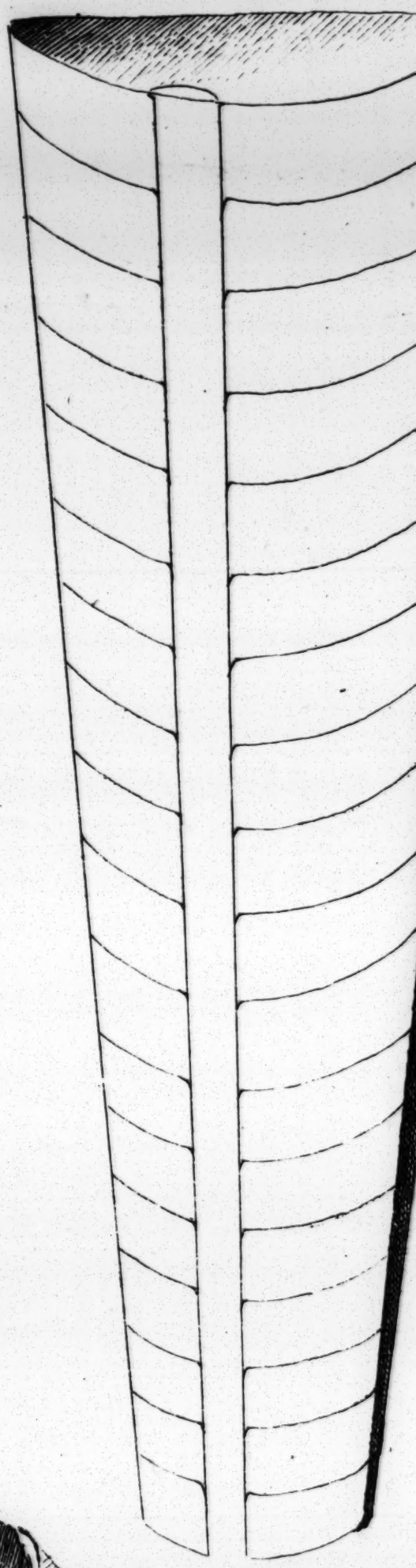
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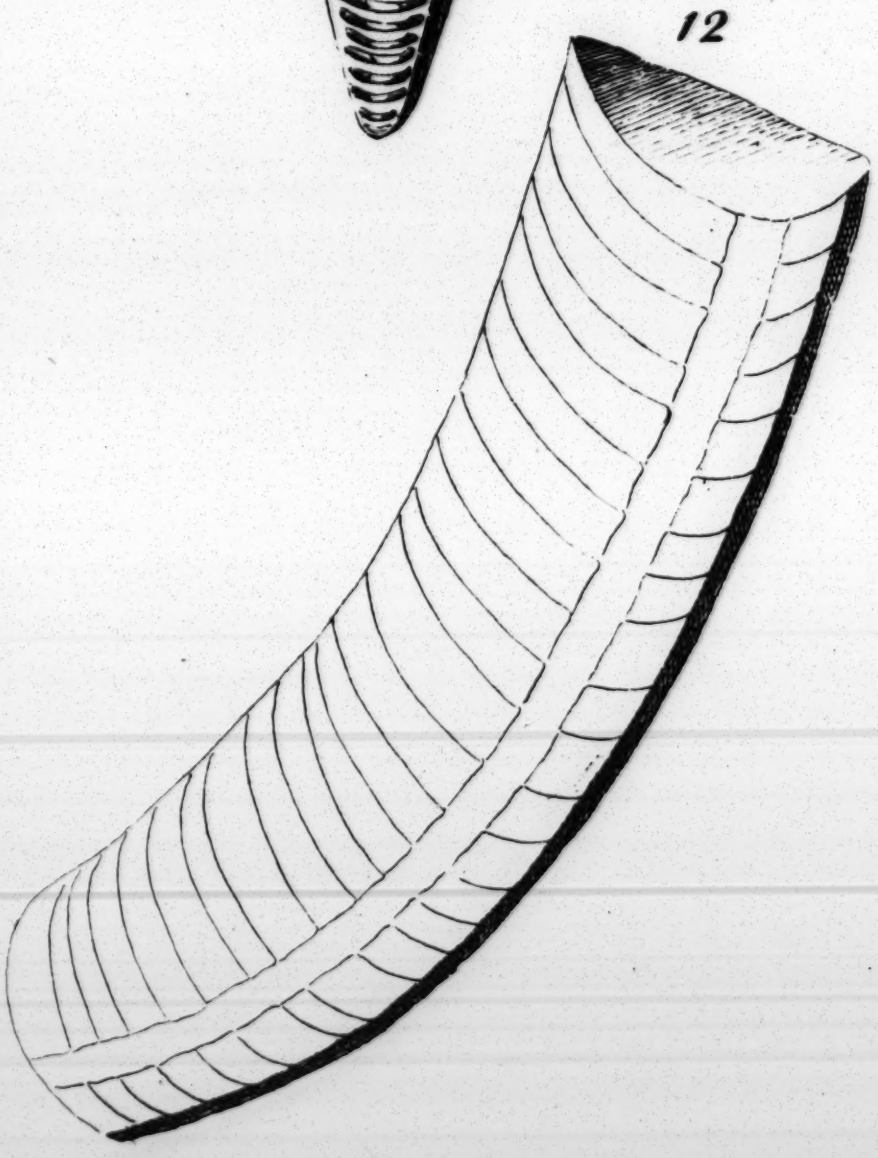
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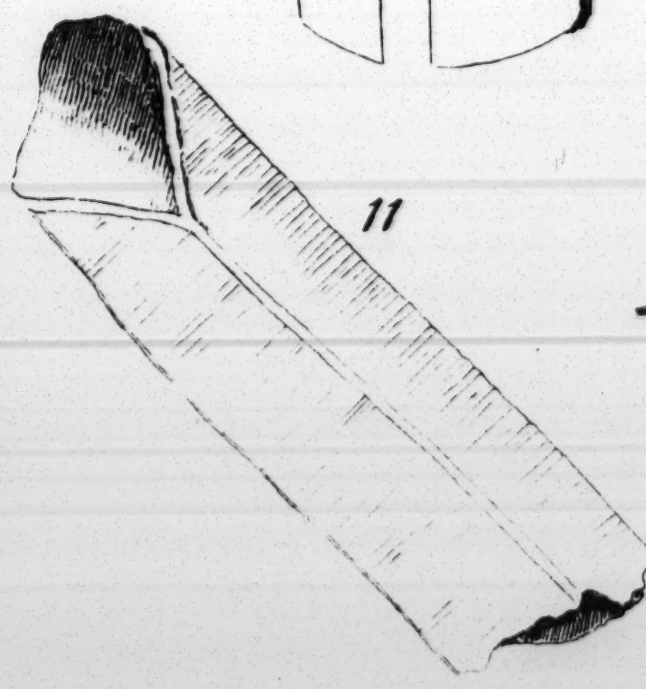
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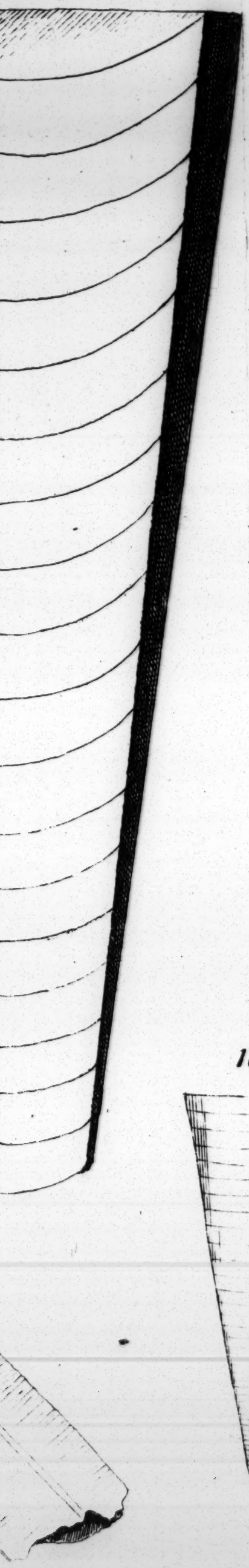
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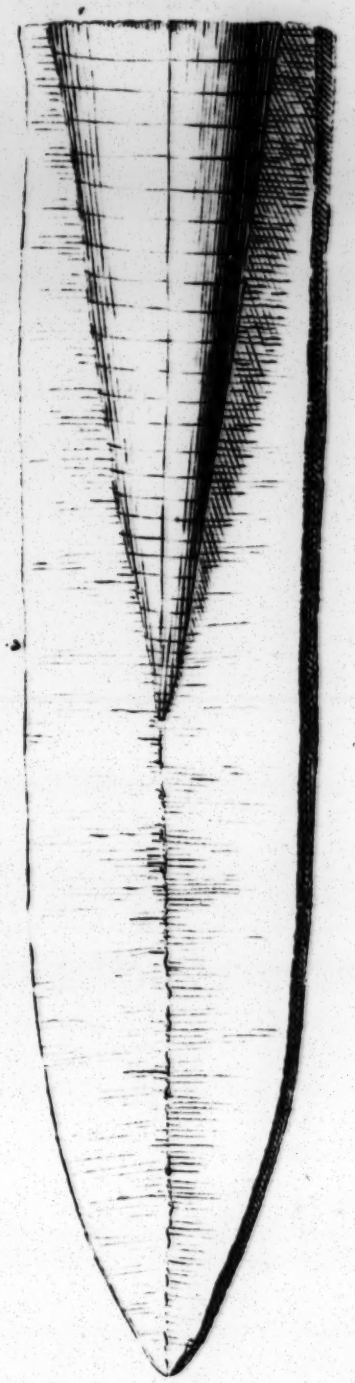
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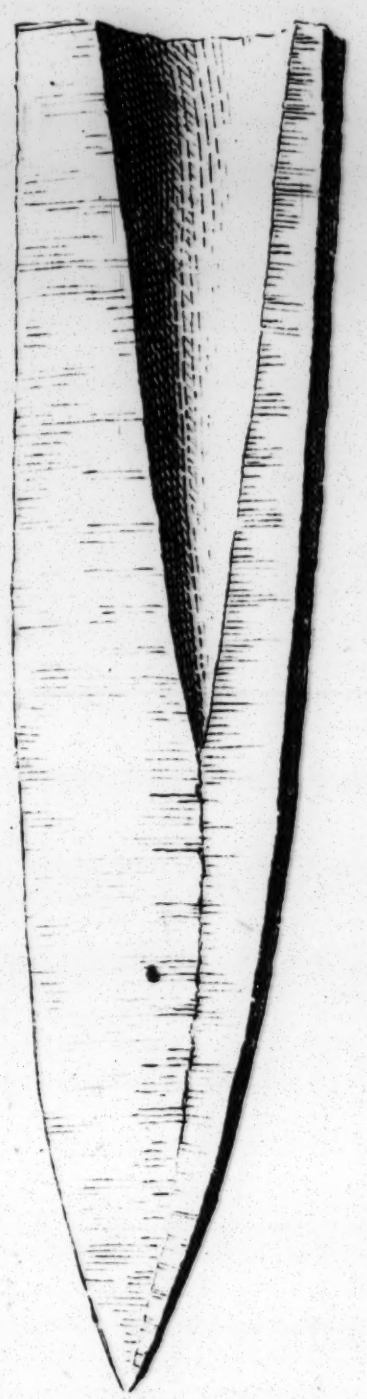
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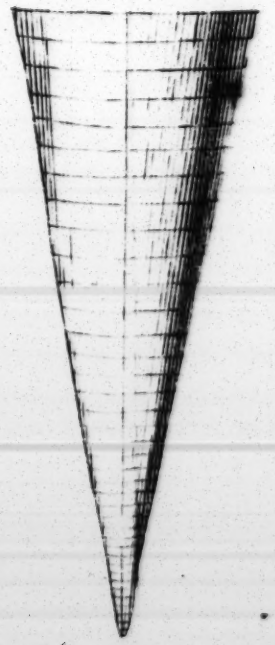
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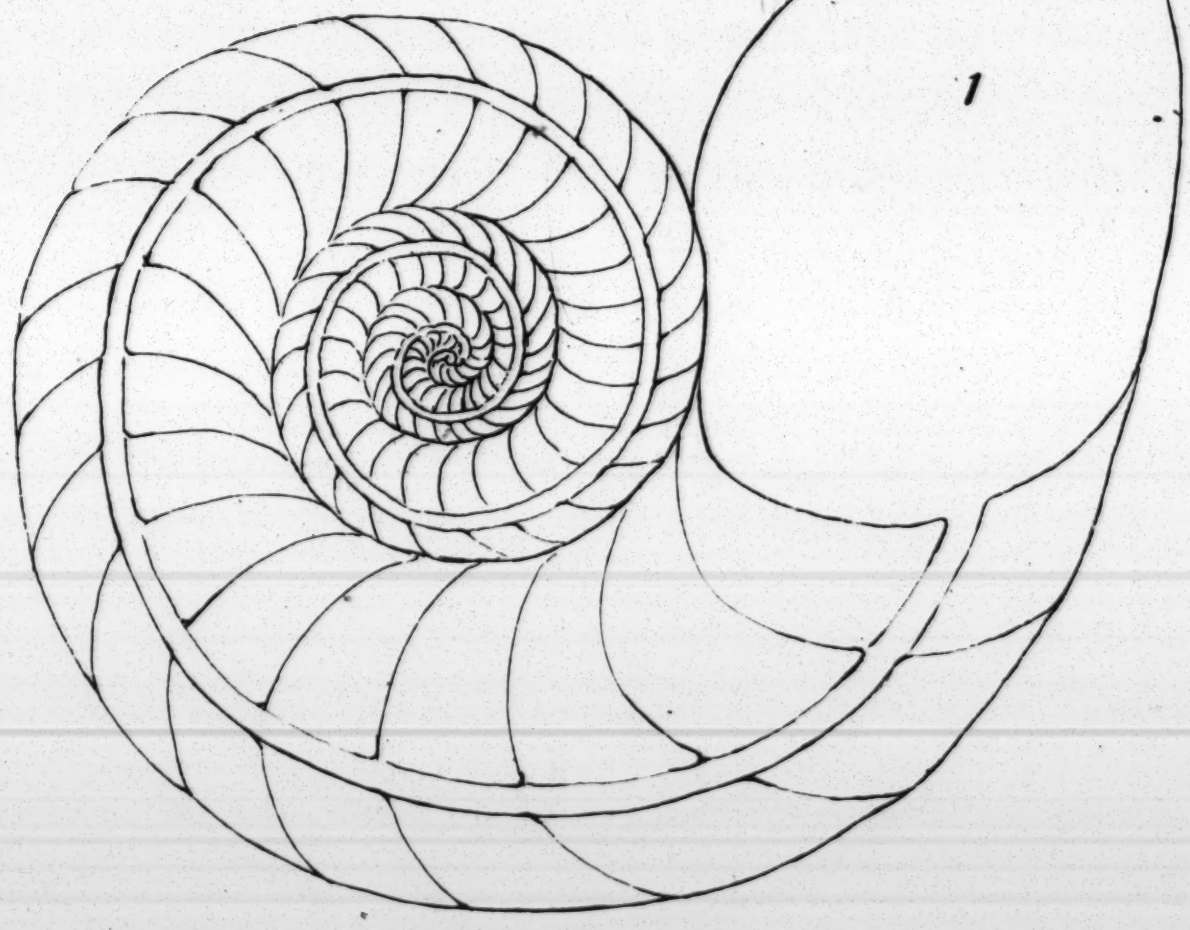


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J. Mynde sculp.

XCVII. *A Diſſertation on the* Belemnites, *in a Letter to* Smart Lethieullier, *Eſquire, F. R. S. by Mr.* Guſtavus Brander, *F. R. S.*

SIR,

Read Nov. 21. 1754. IT is owing to the pleaſure of ſome late converſations with you, upon the ſubject of natural hiſtory in general (in which I am always ſure to profit), that I now take the liberty to offer you my ſentiments concerning the belemnites.

The belemnites is a foſſil, that has hitherto perplex'd the naturaliſts of all countries: it has been treated of by various authors, and in turn been aſcribed to all the three kingdoms of nature; but, apprehending the road to truth ſtill open, I am encouraged to reſume the ſubject once more, and beg leave to deliver it as my opinion, that it belongs to the teſtaceous part of the animal kingdom, and to the family of the nautili. The nautilus, or ſailor, is a concamerated ſhell, with a ſyphunculus running through every cell, ſee *Tab.* XXXIV. *Fig.* 1. The ſyphunculus, and the concamerations, are the generical character of this tribe, and are ſuppoſed to ſerve the animal to buoy up its ſhell, by which means it can ſwim or ſink at pleaſure.

Thoſe that are curv'd are very common both in the recent and foſſil ſtate: the ſtrait ones have hitherto only been met with foſſil, and are common enough in Sweden, Livonia, and ſeveral parts of Germany; and have, by naturaliſts, been called or-

thoceratitæ; nay, I have ſeen ſome in Dr. Maſon's private collection at Cambridge, which he told me were found in England, and, I think, came from Whitby; the character of which being exactly the ſame with the nautili, I make no ſcruple to claſs them together.

Whoſoever will examine nicely bodies of any genera, will have a difficulty to ſay, where they begin, and where they end; the gradation is ſo inſenſible, that they muſt be bewilder'd.

From the orthoceratitæ, which is undoubtedly a ſpecies of nautilus, we gradually proceed to the belemnites. The orthoceratites is a ſtrait concamerated ſhell, ending in a point; ſome of which I have ſeen in ſtone eighteen inches long; ſee *Fig.* 2, 3, 4, 5, and 12.

The nucleus, or alveolus, of the belemnites is likewiſe a ſtrait concamerated ſhell or body, exactly reſembling the other in ſhape and ſtructure, but of a ſmaller ſpecies, *Fig.* 6 and 10; and, I think, from the very great analogy, may reaſonably be deemed to be of the ſame family.

In the conic cavity of the belemnites, *Fig.* 7. that contains the nucleus, it is very common to obſerve viſible marks of a ſhelly ſubſtance, as a farther confirmation that the nucleus was a teſtaceous body.

And now a word as to the belemnites itſelf, the counter-part to the other.

It has indeed been truly matter of ſpeculation, how that huge ſolid ſubſtance called the belemnites, excluſive of the nucleus, could be formed; and how it happens that ſome ſhould have the nucleus within them, others not; the cavity to contain the ſame in

ſome

ſome very ſmall, in others ſcarce or not at all viſible.

Theſe, I think, are the main difficulties, all which I ſhall endeavour to elucidate; but muſt firſt acknowlege myſelf obliged to Monſieur de Peyſonnel, and particularly to my worthy and ingenious friend Mr. John Ellis, F. R. S. for their curious obſervations on the nature of coral, upon which this latter part of my hypotheſis is founded. They have, I think, plainly demonſtrated, that many bodies which we always took to be vegetable from their appearance, are really animal, and conſtructed by the polyp; and that ſeveral coralline ſubſtances, hitherto reputed marine plants, are thick beſet with a prodigious quantity of ſeedling-ſhells (too ſmall for the naked eye to ſee), cloſe by each other, as diamonds in a bodkin, ready to come forth in due time out of their ſeveral neſts or cellules; ſee Philoſophical Tranſactions, Vol. 47. pag. 445. and Vol. 48. Tab. V. pag. 115. Whereupon I very humbly ſubmit, if it is not highly probable that the teſtaceous tribe in general are generated like butterflies, and flies of all kinds, the one from a maggot, the other from a polyp? Nay, it appears preſumptive to me, that it muſt be ſo with a great many. On which circumſtances I proceed, that as corals in general, from late obſervation, ſeem to be conſtructed by polypi, what inconſiſtency then to believe them to be the primary ſtate of all or moſt of the teſtaceous tribe? If ſo, it is almoſt beyond a conjecture, that the body called a belemnites (which, on being put into acids, is found to ferment in like manner as coral, and other cretaceous

ceous bodies), is formed likewiſe by a polyp, from which the nucleus ſeems to be the ultimate ſtate.

And I further ſubmit, whether this concamerated ſhell, or body, of which the belemnites is only the habitation, does not appear a ſtrong voucher for this new hypotheſis, by more immediately leading us into the connexion and manner of generation (perhaps particular to the teſtaceous tribe) by remaining within its nidus all its life; whereas the generality quit them ſo ſoon as they are able to ſhift for themſelves.

The polyp is an animal of the vermicular kind; the bodies of ſome are long and ſlender, like a fine ſinew or fibre, extremely tender; and from the head proceeds a variety of claws, or arms, with which it catches its food, and prepares its habitation, or chryſalis. They are without doubt of various ſhapes and textures, according (as I ſuppoſe) to the ſpecies of the animal that is hereafter to proceed from them *; and very wonderful it is, how ſo ſmall, ſo delicate an animal, ſhould be capable of forming ſo large a body as the belemnites! but is not every particular performance of nature equally the ſame to a diligent inquirer? ſo that I conceive the leſs cauſe againſt this opinion; more eſpecially as late authors have pretty clearly demonſtrated, that corals of much greater magnitude are conſtructed by them. Some animals in the terreſtrial part of the creation, naturally aſſociate and herd together. Others again ſeek

* Mr. Henry Baker, F. R. S. has very ingeniouſly delineated a great variety of ſpecies of them, in his treatiſes on the polyp, and upon the microſcope obſerved in freſh water.

ſolitude. The ſame diſpoſitions we find impreſſed on thoſe of the aquatic ſyſtem: then why not among the polypi? as is evidently ſeen from the prodigious variety of coral bodies, where it ſeems in ſome as if thouſands acted in concert together; in others, where each acts for itſelf; of which latter is the belemnites.

The ſhape of the belemnites is generally more or leſs conic, terminating in a point, and of various colours, according to the juices of the ſtratum in which it lay: it has uſually a ſeam or fiſſure, running down the whole length of it, ſometimes filled with a cretaceous ſubſtance. In ſome it is in the middle or axis of the body; in others on one ſide. Its interior conſtitution ſeems compoſed of ſeveral conoid cortices, or cruſts, which, when broken tranſverſely, appear to proceed in ſtriæ or rays from the ſeam or centre; which ſeam I take to have been the habitation or cell of the animal in its polyp ſtate, and in which the body was affixed; or perhaps ſerving as a ſyphunculus, in which was a ligament that proceeded from the nucleus in its perfect ſtate.

The cruſts that it is compoſed of, I ſhould apprehend, denote certain periods in the age of the animal; as the annual rings in a piece of timber, its age: but what thoſe periods are, we are unacquainted with; ſee *Fig.* 7, 8, 9, and 11.

The animals of the teſtaceous tribe in general, as they increaſe in age, increaſe their ſhell in thickneſs, until they have lived their ſtated time, or attained to good old age; and that is done by adding a new cruſt or lamina to it, as ſeveral, if not all the tubuli, the oyſters, and the nautili, witneſs (but theſe obſervations are beſt made on foſſil bodies, when, by great

length

length of time the immediate cauſe of their adheſion becomes weakened, and diſplays to view their texture); after which they grow inactive and dull, the effect of extreme old age, ſuffering other marine bodies, as worms, oyſters, &c. to penetrate and affix themſelves to their outer coat. The like appearances we frequently meet with on belemnitæ, when the animal within, I ſuppoſe, was either wax'd old, or dead; and is an additional proof that the body is of marine origin.

Every one, that has made this part of natural hiſtory his ſtudy, muſt have obſerved, that the minutiæ, or exceeding ſmall foſſil ſhells, very frequently occur, and in the greateſt abundance, eſpecially in fine loam or clay proper to preſerve them: which ſhews that it was ſpring or ſpawning-time when the deluge overwhelm'd the country they were natives of. And that diminiſhes one of the difficulties concerning the belemnites, why ſome have the cavity, others not, or but very ſmall: for may we not attribute theſe ſeveral appearances to the different ages of the animal; as in the ſpring or ſpawning-time, and ſome time after, a thouſand ſmall fiſh appear in the water to one grown to maturity, or ſeedlings on the ſhores of ſhell-fiſh, to one at full growth? and, from a parity of reaſoning, is there not great likelihood to believe that every bellemnites would have had a nucleus, if it had lived? and to ſuppoſe that deficiency to be cauſed by the deluge coming on, in the early part of the ſeaſon, in that ſpot whereof they are natives, before they had attained perfection?

Not that I doubt of there being the ſame variety of ſpecies among the belemnitæ as of any other tribe

in

in the teſtaceous kingdom, we are convinced of that: but let us, for argument ſake, ſuppoſe, that many never would have had a nucleus, that ſtill does not hinder, but that the body itſelf might be formed by a polype, only of a different ſpecies from thoſe, that have the nucleus. And after all, if it ſhould yet be objected, that the animal, though ever ſo ſmall, would retain the exact ſhape of what it would be in future, let them have recourſe to the fine collection of ſhells of my good friend Mr. Arthur Pond, F. R. S. where they may trace many families of ſhells from their minute ſtate, and ſee the regular progreſſions of the animal, till it has attained full growth; by which it will appear, that the reſemblance is often very ſmall between their firſt and laſt ſtate.

The country of the belemnitæ is unknown to us; but there is great probability it is the ſame with that of the conchæ anomiæ in general, and ammonitæ; ſince they are uſually found together, and are well ſuppoſed to be the inhabitants of deep or unknown ſeas, beyond human reach.

And now, Sir, having had ſo frequent occaſion to mention the orthoceratitæ in the courſe of this ſubject, they being here rare and uncommon foſſils, I have given the figures of ſome few ſpecies of them, which perhaps may not be unacceptable, N° 2, 3, 4, 5, and 12.

If it ſhould be aſked, whether they proceed likewiſe from a belemnites? I anſwer, that I ſuppoſe them to proceed from a polype like the reſt, but whether their parent polype formed itſelf a belemnites-like chryſalis or habitation (being a ſtouter animal), is more than I can affirm, although very probable, as the terminating point in them is as ſharp and fine

as the nucleus of the belemnites; and it is obſerved, that all the turbinated ſhells increaſe their circumvolutions from the point, or apex; but that is not the immediate buſineſs of the preſent purpoſe, as nature has many ways to compaſs her ends. My deſign will be anſwered, if it ſhall only be thought, that I have evinced the belemnites to be an animal production, formed by a polype, as other coralline bodies; and its nucleus to be a concamerated teſtaceous body, of the nautili genus, proceeding therefrom.

Such is my hypotheſis of this hitherto not ſufficiently explained body; which I candidly ſubmit to better judgments, to approve or reject, as reaſon ſhall appear. I am, with perfect regard,

SIR,

London, Nov. 14, 1754.

Your moſt obedient humble ſervant,

Guſtavus Brander.

Deſcription of the PLATE.

Nº 1. A ſection of a common nautilus.
2, 3, 5, and 12. Sections of orthoceratitæ.
4. An orthoceratites intire.
6. A ſection of a belemnites, with the nucleus.
7. Ditto, without the nucleus.
8. An oblique ſection of a belemnites, to ſhew the inward ſtructure.
9. A belemnites without a cavity, only a ſmall perforation.
10. A nucleus of a belemnites.
11. A belemnites, with a very ſmall cavity.

XCVIII. *Some Obſervations upon the Agaric, lately applied after Amputations, with regard to the determining its Species. By Mr.* William Watſon, *F. R. S.*

Read Dec. 5, 1754. THE agaric lately applied as a ſtyptic after amputations, and which has been brought from France, I do not believe to be the common agaric of the oak, as has been imagined by the French ſurgeons. What is called the common agaric is a paraſitical plant, found growing upon the oak, and upon ſeveral other trees; and is denominated by Caſpar Bauhin, *Fungus in caudicibus naſcens, unguis equini figurâ*; of which touchwood or ſpunk, and the *amadoue ordinaire* of the French, is uſually prepared. I think it impoſſible, by any proceſs, to prepare from the common agaric a ſubſtance perfectly ſimilar to the French agaric, which exactly anſwers the deſcription, which Breynius gives of the *Fungus coriaceus quercinus hæmatodes*, and of which we find the following * account in the *Ephemerides naturæ curioſorum*. " In medio quercus, " ſays that author, medullæ velut ſubucula circum- " volutus, inventus eſt: tener erat, mollis, flexilis, " planus, nonnihil rugoſus, eâ longitudine et lati- " tudine, ut colobium ex eodem commodé conficere " licuiſſet: colore pallido ad flavum nonnihil ten- " dente; itemque ſubſtantiâ, tractatione, et figurâ

* An. 4, et 5. Obſ. 150.

" alutæ ex pelle caprinâ confectæ ſimilior quam " ovum ovo, niſi quod craſſior, materiâ minus com- " pactâ; intus præterea paſſim duriuſcula & rotunda " tubercula, in manus magis quam in oculos inter- " currentia occultante. Initio guſtatus vis ſtatim " quædam attractiva apparebat. Reperitur et in aliis " arboribus." Breynius further ſays, that this vegetable has been recommended as a moſt excellent remedy to ſtop the bleeding at the noſe.

Mr. Ray, in his *Synopſis ſtirpium Britannicarum*, upon the authority of the late Dr. William Sherard, ſays, that this *Fungus coriaceus quercinus hæmatodes* is found upon putrid oaks in Ireland, where it is called oak-leather; and that the country people there collect and preſerve it to dreſs ulcers with. Dr. Sherard makes no doubt but that it may be found in England; and Mr. Ray had even ſome ſent him from the late Dr. Eales in Hertfordſhire. Dr. Richardſon, in Yorkſhire, found it growing upon the aſh; and Dr. Dillenius further obſerves, that in Virginia it is uſed as leather, to ſpread plaiſters upon; and that, beſides its being a ſoft ſubſtance, ſitting eaſy upon the afflicted part, it has a healing property.

Thus much I thought proper to lay before the Royal Society, in order that thoſe members of it, who correſpond with the French ſurgeons, may make ſome further inquiry into the nature, as well as the preparation, if any, of this uſeful vegetable.

William Watſon.

XCIX.

XCIX. *Two Letters concerning the Uſe of Agaric, as a Styptic.*

LETTER I.

From Mr. Joſeph Warner, *F. R. S. and Surgeon to* Guy's *Hoſpital, to* Thomas Birch, *D. D. Secr. R. S.*

SIR, Hatton-Garden, Decem. 3, 1754.

Read Dec. 5. 1754. THE incloſed paper is a letter, which I received a few days ago, by the hands of William Fellowes, Eſq; a very worthy member of the Royal Society; the contents of which I am deſirous ſhould be communicated to that learned body, as they tend to give a farther confirmation of the good effects of the agaric of the oak, in ſtopping of bleedings after a diviſion of ſome of the moſt capital arteries. At the ſame time, I have taken the liberty of adding, that I have continued to make uſe of this application with the greateſt ſucceſs; and for many reaſons do continue to be of opinion, that it is one of the moſt valuable acquiſitions, that has at any time been made in our profeſſion.

I am, Sir,

Your moſt obedient ſervant,

Joſeph Warner.

LET-

LETTER II.

To Mr. Joſeph Warner.

SIR, Shotteſham, Novem. 10, 1754.

Read Dec. 5, 1754. I Return you thanks for the agaric of the oak, which I received by Mr. Fellowes, and ſend you an account of the effect I have obſerved it has had, according to your deſire, that you may have the ſatisfaction of ſeeing I have made no unprofitable uſe of your favour. Two or three days after I received it, I was deſired by an ingenious ſurgeon to be with him upon buſineſs. I carried ſome of it with me, and he was pleaſed with the opportunity of trying it in an amputation below the knee of a boy of about ten years old. We applied it according to the directions given in your book, which I have read with pleaſure and profit, and the hæmorrhage was intirely ſtopped in ſix minutes. He informed me, that, on the 5th day incluſive, the dreſſings and agaric came all off without force, and left the ſtump in a good digeſted ſtate, without the leaſt appearance of blood; and that the pain, in conſequence of the operation, did not require an anodyne. He cut a boy for the ſtone the ſame day, and a veſſel bleeding rather more than is thought allowable, he applied a very ſmall piece of the agaric, and a ſoft doſſil of lint over it, which, with gentle preſſure of the finger, reſtrained the bleeding in leſs than a minute. My own patient, aged near ſeventy, whom I made trial of it upon, the 23d of the laſt month, in amputating his leg below the knee, appeared as

proper a ſubject to eſtabliſh the credit of this new ſtyptic as could be produced, if it failed not in its efficacy; there being in him a great depravation of the fluids, and a general relaxation of the ſolids; and he had an ulcer upon his leg, of the phagedænic kind, of many years ſtanding, attended with carious bones. Under theſe diſcouraging circumſtances he applied to me about a month before, and begged of me to take off his leg; the pain, he ſaid, being ſo violent and continual, that he knew not how to live with it: and tho' I looked upon him as a very bad ſubject for the operation, yet I did not care to deny his moſt earneſt requeſt, ſeeing no other poſſible means left of affording relief in his miſerable condition. Conſidering the rigidity of the fibres in an old perſon, and that their natural contractile power, evident in the diviſion of an artery, muſt be greatly weakened in this caſe, I was afraid, that the agaric, if it ſhould anſwer, would not act ſo expeditiouſly as it did in the other; and that probably we might meet with much more difficulty in reſtraining the hæmorrhage. Therefore, to aſſiſt it all I could, I tacked it to thick compreſſes of lint with pieces of card in the middle, thinking by that means I could apply it more readily, and keep it in ſtronger and cloſer contact with the mouths of the veſſels, if I ſhould find it neceſſary; for indeed I was very ſolicitous for the ſupport of its credit and reputation, my own being connected in ſome meaſure with it, and the patient's welfare alſo depending upon it. I applied moſt of the pieces without being under a neceſſity of having the tourniquet-ligature ſlackened, to ſhew the mouths of the veſſels; then covered the ſtump thick with lint, applied

plied a pledget of tow ſpread with common digeſtive over it, and over that a circular piece of ſtiff paper, to make the preſſure of the palm of the hand more equal. This done, after three or four minutes I deſired my aſſiſtant to ſlacken the tourniquet-ligature; upon which it bled at a great rate, and made ſome of my brethren ſoon imagine, and declare, they thought it would not do in this caſe. I was not without the ſame fears; but we went on with reſolution, and every thing was conducted by us without hurry or confuſion. I deſired to have the tourniquet-ligature let quite looſe, in order to remove, as much as poſſible, all impediment to the reflux of the blood, and made ſtrong compreſſion at the end of the ſtump, upon which the bleeding almoſt inſtantly abated, which was totally ſtopped in about half an hour after; and, in the whole, I believe he did not loſe more than twelve ounces of blood. Now, apprehending that the circular ſtructure of the common bandage, as uſually applied, might produce the ſame inconvenience, which I obſerved aroſe from the tourniquet-ligature before it was quite looſe, if no other attended it, having ſtrong ſuſpicions of ſtrait bandage doing much miſchief; I therefore only put ſeveral ſtrips of common plaiſter, about an inch broad, over the piece of ſtiff paper at the end of the ſtump, to meet and lap over at the top of the knee when bent, and then ſlipped on a barber's woollen cap; which method ſeemed to us to anſwer the purpoſe very well in this, as I have found it do in ſome other amputations. A phyſician, and four ſurgeons of eminence, who were deſirous of ſeeing the effect of the agaric, were preſent at the operation. Four days after I opened the

ſtump,

ſtump, but took away no more of the dreſſings than what were looſe, amongſt which were two or three pieces of the agaric, without any ſigns of freſh bleeding, or viſible pulſation at the ends of the arteries. Two days after I dreſſed it again; the ſtump then appeared well digeſted all over, and had a much better aſpect than could reaſonably be expected in ſuch an unpromiſing ſubject; and now appearances are ſo favourable as to give me hopes of his recovery, tho' I am not without diſtant fears, which I am guarding againſt as much as I can. I have lately been informed by very intelligent people, in the county of Suffolk, that they gather an excreſcence, growing there plentifully upon oaks, and call it *Funk*, which, impregnated with nitre, is uſed as a match to light pipes; from whence I conclude it is the ſame with the *Fungus igniarius*, mentioned in the Mem. de l'Acad. Roy. de Chirurgie, and have herein incloſed a ſmall piece of it for your examination, which appears to me of the ſame kind with the agaric I had from you. Though the chief uſe made of it in Suffolk is by ſmoakers, yet 'tis often uſed, they aſſure me, to ſtop bleeding, and cure ſmall wounds, and has been ſo for time immemorial. If I have treſpaſſed too much upon your time in this detail, I beg you would excuſe me, and for the freedom, with which I write, as if I was acquainted with you: but the ſubject appears to me a matter of that intereſting nature to all mankind, that I think it ſhould and muſt engage the moſt ſerious regard and attention of every one, to ſhew his inclination in ſupport of it what he can; and I hope the ſucceſs of future trials, in your conſpicuous ſtation, will convince the public,

that this new ſtyptic, which creates no pain (unleſs what may ſometimes proceed from the neceſſary degree of preſſure in the application of it), may be relied upon in all caſes, where the needle and ligature has hitherto been thought the leaſt painful, and only ſecure means of ſtopping blood in the arteries.

A happy diſcovery indeed, ſhould this prove the conſequence of it, as I heartily wiſh it may, for the eaſe and benefit of the miſerable part of mankind, who fall under our hands; and am,

SIR,

Your moſt obliged humble ſervant,

B. Gooch.

C. *Extract of a Letter from Monſ.* Bonnet, *F. R. S. of* Geneva, *to* John Clephane, *M. D. F. R. S. tranſlated from the* French.

Geneva, June 3, 1754.

Read Dec. 12, 1754.

THE inoculation of the ſmall-pox continues to be attended with the greateſt ſucceſs in our city. Of ſeventy, who were inoculated, there was not one in any danger. Lauſanne has been as it were forced to imitate us; and we hope, that this excellent method, which we received from England, will ſpread itſelf from one place to another, for the good of mankind.

Monſ. de la Condamine has read to the Royal Academy of Sciences at Paris a diſſertation upon this

this ſubject, which was greatly applauded. But I preſume, that the French will be a long time in adopting the practice of inoculation. The clergy there throw a terrible obſtacle in its way.

CI. *Extract of a Letter from* Conſtantinople, *of the* 16*th* September 1754, *from* Murdock Mackenzie, *M. D. concerning the late Earthquake there.*

Read Dec. 14, 1754. ON the 2d inſtant we had a terrible ſhock of an earthquake about three-quarters after nine at night, which moved from eaſt to weſt, and has done a great deal of miſchief here, and in the neighbourhood. I ſhall only mention what I have ſeen.

Four of the ſeven towers are much hurt; one of them, which is an octagon, has two of its ſides thrown down from top to bottom. It is ſaid ſeveral of the Janiſaries, who were upon guard there, are killed. The three other of the four are much ſhatter'd, and part of the walls fallen down. All the turrets upon the city-wall, from the ſeven towers to the Adrianople-gate, are much ſhatter'd, though none fallen: all the cupolas of the portico of Sultan Mahomet the IId's moſque are thrown down: the Sickergee Han, a ſtrong ſtone building near the above moſque, is quite deſtroyed: ſome part of the wall of the Cara Han is thrown down: one bagnio is quite fallen, and many people ſaid to be deſtroyed in it. The Cautirligee Han is quite down; and the

Vizir Han much ſhatter'd: ſeven minarets (columns from whence the people are called to prayers) of ſmall moſques are thrown down: the moſque called Little Santa Sophia is much damaged, and the priſon of Galata is quite down, and all the priſoners buried in its ruins. There has been much damage done at Balat (a large large ſuburb), Scutari, and upon the canal; and there are bad accounts from Nicomedia, but none well avouched. There have been ſeveral ſmall ſhocks felt ſince, but none have done any harme.

Some ſay there were 2000 people deſtroyed by this calamity, in the town and ſuburbs; ſome 900; and others reduce them to about ſixty, who, by what I have ſeen, are nearer the truth.

The Shock at Smyrna, in the year 1739, which I alſo felt, was much ſtronger.

On the 6th, about nine at night, there appeared a cloud due weſt, when it began to lighten and thunder, and the thunder continued, without any interval, till half an hour paſt ten, moving gradually to north-eaſt, where it ceaſed, and the night was very ſerene and calm after it. About ten, when the thunder was north of us, it rained for a quarter of an hour very heavily, then became clear, and all the ſtars appeared. Such a peal of thunder I never heard in any country; for I can aver, that it did not ſtop a minute in an hour and an half's time.

Another letter, dated October 1, ſays, that a Tartar was arrived expreſs from Armenia, in twenty days, with advice, that the city of Sivas, one of the Sebaſtia's of the antients, was quite deſtroyed by an earthquake, on the ſame night, in which that was felt

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at Conſtantinople; and that a lake of freſh water is riſen where the town ſunk. The earthquake was felt at Angora and Smyrna, but there was no notice, they had felt any thing of it at Aleppo, though there were letters from thence as freſh as that time.

CII. *Extract of a Letter from* Camillo Paderni, *Keeper of the* Herculaneum *Muſeum, to* Thomas Hollis, *Eſq; relating to the late Diſcoveries at* Herculaneum.

Naples, Oct. 18, 1754.

Read Dec. 12. 1754. I Shall now proceed to give you ſome little account of our late diſcoveries at *Herculaneum*. We are going on to dig in two places, one towards the road *della Torre del Grego*; and the other nearer to the neighbourhood of the royal palace at *Portici*, directly under the wood belonging to the church of the *Auguſtines*. This latter place has afforded many things, and will, as we hope, furniſh many more. Pleaſe only to reflect, in what manner the perſons employed are obliged to work through the ſubterraneous paſſages, and how hard they find the *lava*; and then you may judge why they advance ſo ſlowly. The firſt thing here diſcovered was a garden, in which were found divers ſtatues of marble of excellent Greek artiſts. This route led us towards a palace, which lay near the garden. But before they arrived at the palace, they came to a long ſquare, which formed a kind of *Forum*, and was adorned throughout with columns of ſtucco; in the middle of which was a bath. At the

the ſeveral angles of the ſquare was a *terminus* of marble, and upon every one of thoſe ſtood a buſt of bronze, of Greek workmanſhip, one of which had on it the name of the artiſt, ΑΠΟΛΛΩΝΙΟΣ ΑΡΧΙΟΥ ΑΘΗΝΑΙΟΣ. A ſmall fountain was placed before each *terminus*, which was conſtructed in the following manner: Level with the pavement was a vaſe to receive the water, which fell from above: in the middle of this vaſe was a ſtand of baluſtrade work, to ſupport another marble vaſe. This ſecond vaſe was ſquare on the outſide, and circular within, where it had the appearance of a ſcallop-ſhell; in the centre whereof was the ſpout, which threw up the water, that was ſupplied by leaden pipes incloſed within the balluſtrades. Among the columns, which adorned the bath, were alternately placed a ſtatue of bronze, and a buſt of the ſame metal, at the equal diſtance of a certain number of palms. It is true, that more ſtatues have been found among theſe columns than buſts; but however, we hope in our progreſs to find a like number of theſe latter. The ſtatues, taken out from April 15 to September 30, are in number ſeven, near the height of ſix Neapolitan palms; except one of them, which is much larger, and of an excellent expreſſion. This repreſents a fawn lying down, who appears to be drunk, reſting upon the goat-ſkin, in which they antiently put wine. Two other of theſe ſtatues are of young men, and three of nymphs; all of middling workmanſhip. September 27, I went myſelf to take out a head in bronze, which proved to be that of Seneca, and the fineſt, that has hitherto appeared; being, I aſſure you, Sir,

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as excellent a performance as can well be conceived. The artiſt has choſen to repreſent him in all thoſe agonies, which the poor man muſt have ſuffered when ready to expire; for one perceives in it every paſſion, that would naturally ariſe in the countenance of a perſon in that ſituation. We are going on with all poſſible diſpatch to examine the whole ſpot before deſcribed; though we have obſerved, that in five places, where we might have expected to meet with buſts or ſtatues, the antients had been digging before us, and taken them away. The method, whereby they regulated their ſearches, ſeems to have been this: where the ground was pretty eaſy to work, they dug through it; and where they met with the ſolid *lava*, they deſiſted. But whether they were in want of money, or of hands, they certainly did not perfect their intention; as is plain from the ſtatues, which we have found. Our greateſt hopes are from the palace itſelf, which is of a very large extent. As yet we have only entered into one room, the floor of which is formed of moſaic work, not unelegant. It appears to have been a library, adorned with preſſes, inlaid with different ſorts of wood, diſpoſed in rows; at the top of which were cornices, as in our own times. I was buried in this ſpot more than twelve days, to carry off the volumes found there; many of which were ſo periſhed, that it was impoſſible to remove them. Thoſe, which I took away, amounted to the number of three hundred thirty-ſeven, all of them at preſent uncapable of being opened. Theſe are all written in Greek characters. While I was buſy in this work, I obſerved a large bundle, which, from the ſize, I imagined

gined muſt contain more than a ſingle volume. I tried with the utmoſt care to get it out, but could not, from the damp and weight of it. However I perceived, that it conſiſted of about eighteen volumes, each of which was in length a palm and three Neapolitan inches; being the longeſt hitherto diſcovered. They were wrapped about with the bark of a tree, and covered at each end with a piece of wood. All theſe were written in Latin, as appears by a few words, which broke off from them. I was in hopes to have got ſomething out of them, but they are in a worſe condition than the Greek. From the latter the public will ſee ſome intire columns, having myſelf had the good fortune to extract two, and many other fine fragments. Of all theſe an account is drawing up, which will be publiſhed together with the other Greek characters, now engraving on copper-plates, and afterwards make a ſeparate work by themſelves. The perſon, who is to give this account, is of the firſt diſtinction among the learned at Naples; ſo that there is no doubt but it will be very ſatisfactory. We are likewiſe in hopes, that in a ſhort time the catalogue of the paintings will be publiſhed; to which, it is not unlikely, will be added that of the ſtatues and bronzes, if the volume admit of it; in order thereby to unite them into one work, and better ſatisfy the curious. In the mean while they are very buſy in engraving the paintings themſelves; of which the king has reſolved to publiſh the firſt volume ſpeedily, a great number of the plates being already finiſhed. At preſent the monk, who was ſent for from Rome, to try to open the former manuſcripts, has begun to give us ſome hopes

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in reſpect to one of them. Thoſe, which I have opened, are philoſophical tracts, the ſubjects of which are known to me; but I am not at liberty to be more explicit. When they are publiſhed, they ſhall be immediately conveyed to you. The firſt *papyri*, of which I formerly acquainted you, were in a ſeparate room, adjoining to the before-mentioned palace.

CIII. *Extract of a Letter from Sir* James Gray, *Bart. his Majeſty's Envoy to the King of* Naples, *to the Right Honourable Sir* Thomas Robinſon, *Knight of the Bath, one of his Majeſty's Secretaries of State, relating to the ſame Diſcoveries at* Herculaneum.

Naples, October 29, 1754.

Read Dec. 12, 1754.

SEveral curious and valuable things are daily found in the mine of antiquities at Portici. They have lately met with more rolls of *Papyri* of different lengths and ſizes, ſome with the *Umbilicus* remaining in them; the greater part are Greek in ſmall capitals. The Canonico Mazocchi, who is much eſteemed for his learning and knowlege of antiquity, is employed in copying and explaining five intire columns, that have been lately unrolled off one of the *Papyri*, which gives ſome hopes of farther diſcoveries. This manuſcript treats of muſic and poetry. The Epicurean philoſophy

loſophy is the ſubject of another fragment, a ſmall buſt of Epicurus, with his name in Greek characters, was found in the ſame room, and was poſſibly the ornament of that part of the library, where the writings in favour of his principles were kept; and it may alſo be ſuppoſed, that ſome other heads of philoſophers, found in the ſame room, were placed with the ſame taſte and propriety.

Laſt week were found two fine bronze heads, of excellent workmanſhip, one of Seneca, and another of a captive king. The king ſpares no expence in recovering and preſerving theſe valuable remains. In order to ſatisfy the curioſity of the public, he has ordered a catalogue to be printed, with ſome deſigns of the principal ſtatues and paintings, which will be publiſhed ſoon. A more exact account of theſe diſcoveries will ſome time or other be given by Monſignor Baiardi, who, in three large quarto volumes already printed, has not finiſhed his introduction.

CIV. *An Account of ſome Trials to keep Water and Fiſh ſweet, with Lime-water.* *By* Stephen Hales, *D. D. F. R. S.*

Read Dec. 19, 1754. DR. Alſton, of Edinburgh, having found, that the ſmall proportion of a pound of ſlack'd-lime in a hogſhead of water, ſtirring it, effectually preſerved the water ſweet, not only in a glaſs or earthen veſſels, but alſo in a new oaken veſſel,

April

April 9, I put into a ſeven gallon caſk of water, in the proportion of a pound to a hogſhead, ſome white marble lime; which was what they call ſweated, that is wrapp'd in dung, without which ſweating, it is ſaid, that it will not be reduced to lime.

April 26, It had ſome taſte of the wood, and a ſmall degree of ill ſmell, which being ſomething more ſo on July 27, it was then poured away.

June 15, I put into an 18 gallon caſk 18 ounces of unſlacked ſtone-lime, made of very hard ſtone of the Clee-hills in Shropſhire; that is, in the proportion of four pounds and an half of lime to a hogſhead of 72 gallons.

June 25, The water was ſweet, but had a diſagreeable taſte of the caſk, and continued the ſame Auguſt 24; but October 17 the taſte was ſomething worſe. And, November 12, there ſeemed to be a very ſmall degree of a putrid ſmell and taſte. But the prevailing diſagreeable taſte was from the wood of the caſk, which diſcoloured the water in ſome degree.

I put alſo into a nine gallon caſk two ounces of the ſame unſlack'd ſtone-lime to a gallon; which was in the proportion of nine pounds to the hogſhead, and found it much the ſame all along as the former.

With chalk-lime, in the proportion of two pounds to a hogſhead, it ſoon ſtunk much, and continued ſo to do for four months. This was Thames-water, taken up below London-bridge, which is well known to grow ſweet again, after having ſtunk for ſome time. So that chalk-lime (almoſt the only ſort in

uſe here), will not preſerve water from putrefaction: though ſtone-lime, as Dr. Alſton has happily diſcovered, does preſerve water in a great meaſure from the great degrees of putrefaction it is ſubject to, and therefore may be very ſerviceable at ſea.

Being informed, by one, who had been in the Eaſt-Indies, that native mineral ſulphur had been found to keep water ſweet there in earthen-jars, at land, and alſo at ſea, (but I know not whether in jars, probably in caſks),

April 2, I put into a kilderkin, or eighteen gallons of pure pond-water, a pound of native mineral ſulphur, in ſeven lumps.

April 26, Sweet.

May 3, Began manifeſtly to ſtink.

May 7, Stunk much, and was poured away.

May 8, The kilderkin being ſcalded, and made ſweet, it was filled again with the ſame pond-water, and ſix pounds of native mineral ſulphur put into it.

July 27, It was ſweet.

October 17, It was diſcoloured, and ſomewhat in a ſmall degree fetid.

November 12, The ſame.

Hence native mineral ſulphur may be of ſervice to preſerve water from great degrees of putrefaction at ſea.

Dr. Alſton having written me word, that he found fiſh would continue ſweet in lime-water for ſeven and more weeks,

April 19, I put four gudgeons into white marble lime-water.

May 10, They were ſweet; but on boiling one of them, the fleſh, though ſweet, was reduced to be ſoft pap.

And

And Monſieur Clairaut, who was at Lapland, to meaſure a degree of the earth, told me on this occaſion, that the fiſh, which they there kept long dried, were thus pappy when boiled, but not unwholſome.

May 22, They ſmelt ſweet, and were firm to the feeling; but, on boiling one of them, it diſſolved away like anchovy.

June 12, Another of the gudgeons, though ſweet and firm to the touch, being put into new-made ſtone-lime-water, which was only milk-warm, diſſolved alſo, and the bones of the head were rotten and brittle.

June 18, Two ſmall eels, ſkinned, were put into ſtone lime-water.

June 22, One of them, which was firm to handle, when boiled was ſoft and pappy.

June 25, The other eel was the ſame when boiled.

In order to try whether the lime, which adhered to, or had ſoaked into, the fleſh of the fiſh, which had lain in lime-water, had the quality of thus diſſolving the texture of the fleſh in boiling, I boiled a ſmall eel, and a morſel of mutton, for ten minutes, in ſtone lime-water, when they were boiled enough, and were of a due degree of firmneſs, and not pappy.

A like eel, boiled in well-water, was boiled enough in five minutes.

Hence it appears, that the lime does not, in boiling ſo ſhort a time, diſſolve the texture of the fleſh into a pap, which muſt therefore be the effect of unfetid putrefaction.

But lime-water made of chalk-lime has very little of an antiſeptic quality.

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For laſt year, in the month of May, I put ſome gudgeons, and an eel, into our common lime-water, and in ſeven days boiled one of the gudgeons, but found it too putrid to eat.

After twenty-eight days I boiled another, and it diſſolved almoſt into inſenſible parts; which ſhews, that it was much putrefied.

Dr. Alſton likewiſe informed me, that he put a piece of veal in pounded or ſlack'd ſtone-lime, which in a week became tough and dry. I put a piece of veal, from half to three-quarters of an inch thick, into chalk-lime, on May the 10th, and on the 31ſt of the ſame month it had a putrid ſmell, and was in the middle red and raw, with a thin hard outſide.

Having communicated theſe experiments to Dr. Pringle (whoſe trials having been made with chalk lime-water, which is in common uſe here, agreed with the laſt of mine), he obſerved, that the difference between ſtone-lime-water and chalk-lime-water might probably conſiſt in this: The chalk, before calcination, being a highly ſeptic ſubſtance *, if ſome of its particles were not fully calcined, theſe, by mixing with the water, would impart to it ſome degree of a putrefying quality, contrary to that virtue the water receives from ſuch parts, as are ſufficiently burnt. That the ſame would be the caſe of ſhells, alſo ſeptics; and therefore that the lime-water, made either of chalk or ſhells, would prove more or leſs antiſeptic, or even continue ſeptic, accord-

* Obſerv. on the diſeaſes of the army, 1ſt Ed. p. 390.

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ing to the degree of calcination. He added, that as all his experiments relating to the antiſeptic quality of lime-water were made in a furnace, heated to the degree of human blood, a circumſtance, which he had marked in his Obſervations *, the uncalcined parts of the lime would in that ſtate become more active in promoting putrefaction, than when the trials were made in cold water.

And indeed it muſt be owned, that when any experiments are made on medicinal ſubſtances out of the body, the nearer we can make them to the heat of the blood, and to other circumſtances thoſe ſubſtances muſt undergo in the firſt paſſages, the more juſt the inferences will be, that are drawn from thoſe experiments.

In regard to that quality of lime-water, in preſerving fiſh longer ſweet than fleſh, Dr. Pringle took notice, that he doubted it was a common miſtake to account fiſh a more corruptible ſubſtance than the fleſh of land animals. For although fiſh might become ſooner ſtale for eating than moſt fleſh-meats, yet that fiſh did not ſo ſoon riſe to a rank degree of putrefaction as fleſh; and therefore that the former would be kept longer tolerably ſweet than the latter by any kind of antiſeptic.

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* To one of the experiments preceding that upon the lime-water, the author ſubjoins the following note: "All the following "experiments, whether made in the lamp-furnace, or by the fire, "were in a degree of heat equal to that of the human blood, "viz. 100 deg. of Fahrenheit's ſcale." p. 383.

CV. *Medical and Chemical Obſervations upon Antimony.* *By* John Huxham, *M. D. F. R. S.*

Read Dec. 5, 12, 19, 1754.

NOT above two centuries ago, a phyſician, who preſcrib'd antimonials, was expelled the faculty; nay, at Rome, any one, who uſed Pulvis Cornachini, incurred the penalty of being ſent to the gallies, on account of the antimonium diaphoreticum, that was in it. Now, on the contrary, antimony, in ſome form or other, is the grand catholicon, and uſed by dabblers, as well as doctors, in phyſic. It is without all doubt a moſt excellent mineral, when duly prepared, and judiciouſly adminiſtered.

But whoever would give antimonial medicines with ſafety and ſucceſs, ſhould be well acquainted with the analyſis of that mineral, and its component principles; ſhould know what different combinations, preparations, and doſes of them, will affect: otherwiſe it may prove a poiſon, inſtead of a remedy. For who would imagine, without ſufficient experience, that ſix or eight grains of the liver or regulus of antimony, and even a much leſs quantity of its glaſs, will cauſe the moſt violent vomitings; whereas a drachm or more of the crude, or common depurated antimony of the ſhops may be taken with eaſe and ſafety? But further, if equal quantities of antimony and ſalt-petre are deflagrated and melted together, a very ſtrongly emetic liver of antimony is produced; and yet if three parts of that ſalt, and one of antimony

mony are detonated and calcined in a proper fire, a mere inert calx, or antimonium diaphoreticum, as it is called, comes out, not in the leaſt emetic or cathartic. On the other hand, if only one-eighth of nitre had been fuſed with the antimony, a very mild kind of regulus medicinalis had been the conſequence. So likewiſe, if one part of ſalt of tartar is fluxed with five parts of crude antimony, a very gentle medicinal antimony, or, as more commonly called, regulus medicinalis, is prepared; and yet if two or three parts of fix'd alcali ſalt, and one of the ſame mineral, are melted together, a very draſtic kind of hepar antimonii, and commonly a ſmall quantity of regulus, enſue. Nay, antimony well roaſted, calcin'd, and then flux'd into a glaſs, without the addition of any other body, becomes the moſt virulent emetic in nature: but if this very glaſs is only calcined again by the concentrated rays of the ſun, through a large burning-glaſs, it is turned forthwith into an inactive calx, or a ſort of antimonium diaphoreticum. The ſame is effected by burning the vitrum antimonii with about an equal quantity of nitre.

Theſe are facts, which nothing but repeated experience could inform us of; and yet, however ſtrange they may appear, perhaps, when we come more nearly to examine the matter, we may pretty clearly diſcover the reaſon of them.

The caſe ſeems to be thus: Clean, crude antimony conſiſts of much ſulphur, and a conſiderably greater quantity of reguline metallic parts. The ſulphur, or t leaſt what may be called the external ſulphur, is little or nothing different from com-

mon ſulphur, as Helmont ſurmiſed, and Boerhaave, Hoffman, and Geoffroy, have evidently proved; and I think every one now allows it, however virulent the arſenical ſulphur of antimony, as they called it, was deemed by the antient chemiſts. And this ſulphur, like the common ſulphur, is compounded of a vitriolic acid, and a bituminous or inflammable ſubſtance: and whatever Baſil, Valentine, Charras, and others, talked of the vinegar of antimony, there is no other acid in it than what I have mentioned; nor is there any kind of ſalt in antimony but the vitriolic acid, notwithſtanding the conceit of Maets, Duncan Bornett, &c.

But however abundant the ſulphureous principle is in antimony, the reguline greatly exceeds it, and in truth conſtitutes the very body or eſſence of antimony, and in which alone reſides its draſtic power, and emetic quality, as is ſufficiently ſhewn by Monſieur Geoffroy, and of which, at preſent, I believe few chemiſts doubt; for there is no one preparation of antimony emetic, in which the reguline principle doth not greatly abound: therefore no tincture of antimony, made with rectified ſpirit of wine, is in the leaſt degree emetic.

This reguline ſubſtance, or antimony properly ſo called, is a metallic ſubſtance, *ſui generis*, almoſt totally volatile when urged by a very ſtrong fire, and can by no means hitherto known, be brought to a malleable ſtate. One will have it of a mercurial, another of an arſenical nature; but it ſeems in ſeveral reſpects to differ from either. No pure running mercury can by any known method be drawn from regulus of antimony, or its flowers, with ſal ammoniac,

niac, whatever may have been boaſted by the alchymiſtical viſionaries. The exceeding ſmall quantity, that Monſieur Geoffroy fancied he extracted from it, could never be done again by the very ſame proceſs, materials, and the utmoſt care. Beſides, if the reguline part of antimony was really mercurial, how comes the mercury not to fly off intirely in a ſtrong calcination, eſpecially in ſuch an intenſe heat as that of the burning-glaſs? And if it doth ſo, as ſome have aſſerted, how comes the remaining calx, after ſuffering ſuch a violent action of the fire, to be ſo readily reduced to its priſtine reguline metallic ſtate, merely by the addition of any common phlogiſton, as ſulphur, charcoal-duſt, animal fat, or the like? But indeed mercury in no form will ſtand ſuch an intenſe heat, but is ſoon totally evaporated; and ſo would the regulus, if mercurial; the calx of which however, after having ſtood this fiery trial, is, as I ſaid, eaſily revived into a proper regulus. Pure antimony differs from arſenic in not having the leaſt garlick-ſmell, peculiar to arſenic when burnt. Nor is the antimonial regulus at all ſoluble in water, as arſenic, nor in oil of tartar *per deliquium*, in which however white arſenic almoſt intirely diſſolves. Moreover the conſtituent particles of arſenic and antimony are of quite different figures; the former reſembling two quadrilateral pyramids, join'd baſe to baſe; the latter like needles, as it were, and this particularly in the minuteſt diviſions of antimony by ſolution, ſublimation, *&c.* as is remarkably ſeen in the flowers of antimony *per ſe*, and in the ſolution of its regulus in wine, when viewed by a microſcope on a ſlip of glaſs, or the like: nay, when ſtibium is

revived from the glaſs, or calx antimonii, it reſumes the ſpiculine or needle-like appearance; and when mercurius vitæ is flux'd without any addition, it becomes a ſpiculine regulus, or a mere maſs of ſuch aculei, or needles.

Indeed it ſeems to be from this ſpiculine or needle-like form (which is conſtantly retained by the minuteſt particles of the reguline ſubſtance), that antimonial preparations have their emetic quality. Thus ſaline bodies ſtimulate by their points, or edges; ſublimate corroſive by its ſalts actuated by the weight of the cohering mercury; arſenic by its rigid, ſharp-pointed, heavy particles. For when theſe reguline ſpicula are ſheath'd up in a large quantity of ſulphur, as in the crude antimony, they exert no ſuch power; and if regulus of antimony is melted up with pretty much ſulphur, it becomes quite as inert as the crude mineral. So regulus, or glaſs of antimony, melted with wax, is rendered exceedingly much milder than before. Nay, even the virulent quality of the ſtrongeſt white arſenic may be ſurprizingly taken off, by intimately uniting it with common ſulphur by fuſion.

It ſhould ſeem then, that this ſulphureous covering blunts or ſheaths up the points of theſe acrid bodies, and takes off the irritation, which they would otherwiſe cauſe on the nervous coats of the ſtomach and guts. This is plainly ſeen in the glaſs of antimony with melted wax; which, though in itſelf the moſt violent of all emetics, thus becomes infinitely more gentle, and may be given to adults from four, ſix, or eight grains, to ſixteen, with ſafety, and great advantage in ſome caſes; whereas two or three grains of the powder'd glaſs will excite moſt dreadful vomitings.

ings. But let me add, by the way, that if the vitrum ceratum antimonii, after it hath been long made, is afreſh rubbed to a fine powder, it proves much more draſtic, the wax being rubbed off from it, and leaving its points more naked: ſo that even this ſeems to prove what I juſt now hinted. Hartman's chyliſta, or the old preparation of glaſs of antimony with gum-maſtic, diſſolved in ſpirit of wine, and then evaporating the ſpirit, ſeems to be on the ſame foundation, but, I ſhould imagine, nothing near ſo ſafe: however, I never tried it, nor that other preparation of the vitrum antimonii, by the repeated deflagration of ſpirit of wine in it, which, Geoffroy ſays, may be given ſafely to ten or even twenty grains; the oleoſe part of the ſpirit of wine inveloping or blunting the ſtibiate ſpicula, and reducing the glaſs in ſome meaſure back again to its original antimonial ſtate. 'Tis certain the ſaline-mercurial preparations are rendered much milder by burning ſpirit of wine upon them repeatedly.

The leſs therefore of the external ſulphur adheres to the reguline part of antimony, the more vehement is its operation, and *vice verſa*. Thus in preparing the common liver of antimony, the nitre deflagrates with, and carries off, a great part of the ſulphur, whence the antimonial hepar becomes very ſtrongly emetic (I do not conſider at preſent what the alkalization of the nitre in the proceſs doth further). And 'tis much the ſame in making the common regulus; and, when filings of ſteel are uſed in preparing the martial regulus, it is, that the iron may abſorb the antimonial ſulphur. So likewiſe, in calcining antimony for the glaſs, the ſulphur is firſt

driven

driven off by roaſting, and then more perfectly by melting the reſiduum in a ſtrong fire into a glaſs, whence it acquires a moſt violently emetic power, which notwithſtanding is ſoon totally deſtroyed by re-melting it with much common ſulphur.

But although this groſs external ſulphur is not at all neceſſary to the conſtitution of the reguline, metallic part, an internal or metallic ſulphur appears abſolutely requiſite to the exiſtence of the regulus, as regulus; for when antimony is quite deprived of all its ſulphur, by what is called the humid or dry calcination, it ceaſes to be metallic or antimony. Thus antimony expoſed to the ſtrong concentrated rays of the ſun, becomes an abſolutely inert calx, or caput mortuum, and can never be reduced to its original nature and form, but by the addition of ſome ſulphureous body. The ſame happens, when large quantities of nitre are deflagrated with antimony, till the ſulphur is all burnt off, as in the common antimonium diaphoreticum. 'Tis thus alſo in the humid calcination, when ſtrong ſpirit of vitriol is poured on the reguline maſs, which it tears abroad, and lets looſe the phlogiſton; for this calx likewiſe is not reducible to regulus but by ſome ſulphureous pabulum, in cloſe contact and fuſion with it. The ſtrong ſulphureous ſmell of the oil of vitriol, when forced off by diſtillation, or the like, diſcovers the phlogiſton, and whence it came. Moreover this calx is intirely white, and will not in the leaſt deflagrate with ſalt-petre; an argument of the abſence of any ſulphureous matter. This calx however is ſoon alſo turned into a regulus by the addition of a proper ſulphur, and then deflagrates with nitre as uſual.

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It appears then, that ſome internal metallizing ſulphur (perhaps however little or nothing different from the common external ſulphur), is abſolutely neceſſary to combine the metallic earth together, and even to give it the reguline conſiſtence and form, without which it lies an inactive incoherent heap, utterly devoid of any antimonial virtue. Thus the duſt, or minera, or aſhes of iron, are harmleſs, and may be ſwallowed ſafely; but if formed into knives, or needles, the caſe is infinitely altered. We not only ſee the neceſſity of this internal ſulphureous principle in the compoſition of antimony, but alſo in that of the more perfect metals, which, when calcin'd by the burning-glaſs (or otherwiſe) are not reducible into their proper metallic malleable ſtate, but by the addition of ſome kind of ſulphur, as a bond of union between the disjoined particles of the reſpective metallic earths. But it is pretty remarkable, that let the ſulphur be animal, vegetable, or mineral, fat, coal-aſh, or brimſtone, it equally affects the coalition of the reſpective earths, and the regeneration of each of the different metals; ſo that it ſeems to be ſulphur, as ſulphur, that is only wanted in the recompoſition. In like manner as in vegetables, the conglutinating oil is neceſſary to the coheſion of the ſtamina, which being burnt off, they become a mere duſt. It ſeems alſo to argue, that the difference of metals lies in their different ſpecific metallic earths, as I may ſo call them, and not in the different proportion, combination, and purity of the ſulphureous and mercurial principles, as Monſieur Homberg, and others, have imagined. Alſo I know not how the ſulphur, of the very ſame charcoal, equally

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equally ſerves to re-vivify the aſhes not only of iron, copper, or tin, into their reſpective metallic forms and conſiſtence, but likewiſe even the calx of ſilver or gold. But further, if theſe metallic earths were indeed originally of a mercurial nature, and remain ſo after calcination, how comes their mercury to ſtand ſuch an immenſe heat as that of the burning-glaſs? whereas no known preparation of mercury, much leſs mercury itſelf, will ſtand a third part of the heat without being totally diſſipated, and for this reaſon mercurials, as mercurials, never can be vitrified: but theſe earths, or aſhes of metals, do ſtand this exceeding degree of heat, and are vitrifiable, and therefore not mercurial; and their calx may be reduced to metal again, by the addition of ſome ſulphur: but I believe no one will ſay the charcoal, in the reduction, refunds the mercurial principle to them.

Poſſibly it may here be aſked me, what indication there is of any remaining ſulphur in well purified regulus of antimony, or its glaſs? I anſwer, no one can doubt of its being in the regulus, who knows, that the beſt antimonial regulus will deflagrate with nitre, in a red-hot crucible, and give off more or leſs of ſulphureous ſcoriæ; that flux'd with a fix'd alkali-ſalt, it will form a kind of hepar antimonii; and that ſulphur may be eaſily ſeparated from it, when diſſolved in aqua regia. But this is obvious; the ſulphur indeed is not ſo readily diſcovered in glaſs of antimony, but 'tis evidently there; for when that glaſs is finely powdered, levigated, and digeſted with very ſtrong diſtilled vinegar, it tinges it very highly, and imparts to it an emetic quality: and

this

this tincture, evaporated to an extract, gives off the ſulphureous tincture to rectified ſpirit of wine: but, if this ſame glaſs is thus ſeveral times treated with freſh diſtilled vinegar, or its concentrated ſpirit, at length it neither gives it any tincture, or emetic power, but remains a dead, dark-coloured, inert maſs, all the ſulphur being extracted, and the glaſs reduced to a mere calx. Beſides, even glaſs of antimony will in ſome meaſure deflagrate with nitre; which ſhews, that it ſtill retains ſome of the ſulphureous principle; and, in order to render it mild and innoxious, 'tis neceſſary to correct it by burning off the ſulphur of the glaſs with nitre, or by the burning-glaſs: which is in truth the deſtruction of the metallic conſiſtence in the glaſs.

But it is a much more difficult thing to prove the exiſtence of the reguline ſpicula in the glaſs of antimony, in the very form of which I ſeem, in a great meaſure, to have placed its emetic quality; and yet glaſs of antimony is the moſt violent of all its preparations. I confeſs, the ſpicula, or needles, by no means appear in the glaſs; but they really do ſo, when the glaſs is digeſted, and diſſolved in wine, by laying a drop of the liquor on a plate of glaſs, and then viewing it through a microſcope. And further, vitrum antimonii, reduced to a regulus with a little common ſulphur, appears of a needle-like ſtriated form. And incinerated antimony, when melted with too ſlack a fire, often appears a maſs of half-ſtriated regulus, and half glaſs, ſo little is the difference. The ſalts in common glaſs do not appear, though they are unqueſtionably there, and ſometimes ſo looſely combined with the vitreſcible

earth, that wine, kept in glaſs-bottles, made of ſuch ill-prepared vitreous matter, diſſolves ſome of the ſalts, and thence the wine becomes ill-taſted and unwholſome. Moreover, the preparation of the vitrum ceratum antimonii ſeems not a little to confirm the reality of what I have hinted at; for the wax perhaps doth nothing but ſheath up the pointed reguline particles, when melted with them: and this appears the more probable, as a large quantity of brimſtone, melted with white arſenic, ſheaths up the arſenical ſpicula, and renders them incomparably leſs noxious than before: ſo, with a due quantity of ſulphur, glaſs of antimony itſelf is rendered a very mild kind of regulus.

'Tis exceeding difficult to explain the modus operandi of many medicines. Who can ſay, how a grain or two of crude opium cauſes a profound ſleep? or why a very ſmall doſe of cantharides ſo particularly and ſtrongly affects the urinary paſſages? why two or three grains of elaterium operate with more violence than fifty or ſixty of jalap? or why ſuch a very ſmall quantity of glaſs of antimony excites ſuch dreadful vomitings?

But whether the emetic quality of antimony depends on the ſpiculine form, or not, it certainly lies only in the reguline ſubſtance; for not one of the preparations of that mineral is emetic, but when conſiderably impregnated with reguline particles; which, when not inveloped with too much ſulphur, always exert a vomiting faculty; and this, whether given in ſubſtance, or diſſolved in a proper menſtruum, as wine, cyder, vinegar, or the like. Water, as water, draws nothing from an antimonial regulus,

as neither touching the ſulphureous or metallic part. Spirit of wine hath no manner of effect on the reguline; but a vegetable ſaponaceous acid acts on both, and draws out the real ſubſtance of the regulus, making as it were a very attenuated liquid tartar emetic, or antimonial ſolution. And thus indeed the vegetable acids act on iron or copper; which neither pure water, nor pure ſpirit, will affect, but are moſt readily diſſolved by wine, cyder, juice of lemons, or the like.

This leads me, however, to make the following obſervations: That though ſimple water is ineffectual in drawing off any thing emetic from pure regulus of antimony, yet certainly rain, or river-water, in ſome meaſure, acts on the body of crude antimony, and extracts from it a milky hue, and a ſulphureous antimonial ſmell and taſte, by being digeſted with it in a very gentle heat of the ſun, or fire. And this water, by the way, ſo impregnated, hath been very often found of great ſervice in cutaneous and other diſorders, when drunk freely. Now, as the watery menſtruum doth unqueſtionably take up ſome of the antimonial ſulphur, it may receive therewith likewiſe ſome of the very fine reguline parts attached to the ſulphur; juſt as almoſt all the natural ſulphureous waters hold alſo ſomething, more or leſs, of ſome other mineral principle, as particularly moſt of them ſomething ferrugineous. And further, whatever may be thought of giving crude antimony in ſubſtance; Kunkel, Hoffman, Geoffroy, and many others, aſſert its great utility in ſeveral diſorders; and, I think, I have ſufficient reaſon to be of their opinion. It is ſcarce to be doubted, but that it is of

very great ſervice in ſeveral of the diſeaſes of horſes, cattle, &c. and therefore we may very well ſuppoſe, that ſome of the reguline parts paſs with the ſulphureous into the maſs of their blood; and, by parity of reaſon, antimony may as well find its way through the *Vaſa lactea*, &c. of human bodies, and produce very ſalutary effects. I have, for a great many years, given antimony and quickſilver, rubbed into what I call an antimoniated æthiops, with great advantage, in ſeveral caſes, particularly in cutaneous diſorders, obſtructed ſcrophulous glands, rheumatiſms, &c. when the common æthiops had been found much leſs effectual. This I have long ordered to be kept here as an officinal medicine, and to be prepared of crude antimony, exceedingly fine powdered, p. iii; of pure quickſilver, p. iv; of flowers of ſulphur, p. ii. Theſe are to be rubbed into an impalpable black powder: Doſe from ℈ ſs. to ℈ ii.

It lies in my way here, alſo to mention, that though cinnabar of antimony, ſublimed in the uſual way after the butter of antimony, may be very little different in virtue, or compoſition, from the common factitious cinnabar; yet cinnabar of antimony, as now generally made, with æthiops mineral and crude antimony raiſed together, hath undoubtedly ſome of the reguline parts, as well as the ſulphureous, of antimony, which carry them up; for it is well known to chemiſts, how eaſily the flowers of antimony riſe in the common roaſting, where the fire is much leſs than for ſubliming the cinnabar. Now, every one knows, that theſe flowers are of a reguline nature, are ſtrongly emetic, and may be eaſily reduced to an actual regulus. The abundant ſulphur

indeed

indeed wraps them well up in the cinnabarine preparation: however, I have often obſerved large doſes of cinnabar of antimony create a nauſea and puking, eſpecially on tender ſtomachs. But then, for this very reaſon, this cinnabar of antimony may be, in many caſes, much more effectual than the common factitious, or even than the native cinnabar.

But I come now particularly to make a few obſervations on ſome of the common antimonial preparations.

The common ſtibium, or crude antimony of the ſhops, hath been melted from its groſs ore, into a ſort of conical moulds, or veſſels, like our melting-pots; whence the molten maſs, when taken out cold, ſomewhat reſembles a ſugar-loaf. Now, as in this melting, the more ponderous or metallic part ſubſides to the bottom, or narrow part of the pot, the lighter, or more ſulphureous, remains above; it is a matter of ſome importance in making the regulus antimonii, *&c.* from what part of the cone, or loaf, the antimony is taken; for the nearer the point of the cone the more reguline; and the yield, from a given quantity, thus taken, will be, *cæteris paribus*, much more conſiderable, than if taken near the baſe. This thing ſhould be likewiſe attended to, when antimony is given in ſubſtance, or boiled, or infuſed in diet-drinks, in which it is far from being a uſeleſs ingredient.

I have before taken notice, that when the reguline part of antimony is involved in a great quantity of ſulphur, as in crude antimony, that mineral exerts no emetic or draſtic power: but when the antimony, by long roaſting, or calcination, is depriv'd

of

of great part of this external ſulphur, it acquires more and more of a vomiting quality, as the ſulphur is more and more conſumed. And when this incinerated antimony, as 'tis called, undergoes a further degree of fire, and is fluxed into a regulus, and even yet farther into a kind of glaſs, it is ſo far ſtripped of its ſulphureous covering, that the reguline ſpicula lie as it were naked, and exert the utmoſt violence on the ſtomach, *&c.* And even before it is turned into a proper regulus, or glaſs, it acquires, after a long and proper roaſting, no ſmall degree of an emetic power, as any one will find, who gives it in ſubſtance, or properly digeſted in wine, or cyder. And though I cannot ſay with Boerhaave, that it is violently emetic, yet I know Hoffman is miſtaken, when he pronounces it quite inactive. This incinerated antimony then, being thus far deprived of its external ſulphur by calcination, is farther diveſted of it by being kept fuſed a conſiderable time in a very ſtrong fire, and converted into glaſs; which almoſt intirely conſiſts of reguline parts, as is manifeſt from its being of a much greater ſpecific gravity than crude antimony, or even than its hepar; and though the regulus lies concealed under a glaſſy form, yet it is ſtill very eaſily reduced into a proper regulus, and therefore, as ſuch, communicates a moſt ſtrongly emetic quality to any menſtruum, which the regulus itſelf would impart it to, as well as being in ſubſtance moſt violently vomitive. Nay, in running calcined antimony into glaſs, if the fire is not briſk, and well managed, part of the very ſame molten maſs runs into regulus, and part into glaſs, ſo little is the difference between them.

Indeed,

Indeed regulus of antimony itſelf cannot be prepared but by deſtroying the external ſulphur: for inſtance, eight parts of crude antimony, ſix parts of crude tartar, and three parts of pure nitre, made into a dry fine powder, muſt be thrown, by ſmall ſpoonfuls at a time, into a red-hot crucible, whence a very great deflagration immediately ſucceeds each projection. At the bottom of the maſs, firſt duly melted by a quick ſtrong fire, the regulus is found ſometimes more, ſometimes leſs, as the fire, &c. are managed. 'Tis a neceſſary caution to force down the cruſts, that are apt to form, at the beginning of the fuſion, with an iron rod, or the like. Here a great part of the external antimonial ſulphur is conſumed by the deflagration, and the tartar and nitre being alſo forthwith alkalized by the fire, unite with the remaining ſulphur, and ſo form a kind of hepar ſulphuris, which diſſolves, and takes up likewiſe, much of the reguline ſubſtance; (for liver of ſulphur, melted with any kind of metal, diſſolves it, and even makes a maſs ſoluble in water); ſo that by this proceſs much the greater part of the antimony and ſalts is turned into a ſcoria, or a kind of crocus antimonii, on the top of the regulus; and this eſpecially if the fuſion is long continued. The regulus, thus detached in a great meaſure from the ſulphureous part, becomes very ſtrongly emetic, for the reaſon above aſſigned. Though this is a common method of making regulus of antimony, at leaſt in ſmall quantities; yet it is far from giving ſo large a portion of regulus, as may be had by ſeveral other proceſſes; for it conſumes not only much of the ſulphureous, but alſo of the reguline part, by the violent

lent and repeated deflagrations, which riſe in the form of flame, fume, and flowers, if catched in proper places; and theſe laſt may be eaſily reduced to regulus again. Beſides, the proportion of the ſalts is too great, for, being alcalized, and uniting with the ſulphur, they take up too much of the reguline part, ſo as to leave very little at bottom, if a ſtrong fire is long continued. Much more regulus, in proportion, would have been yielded, if the tartar and nitre had been previouſly fired, and turned into what is called the black flux. But, even in this caſe, the melted maſs ſhould be poured off, or taken from the fire, as ſoon almoſt as it flows very thin; elſe no ſmall part will evaporate by the ſtrength and duration of the fire, which alſo increaſe the proportion of the ſcoria. But a much greater quantity of regulus is produced by melting two parts of clean antimony with one part of iron-filings, or bits of nails, to which alſo one part almoſt of pure dry ſalt-petre ſhould be projected. This proceſs gives almoſt half regulus. Here the nitre in part likewiſe burns off the ſulphur, but 'tis the iron, that chiefly takes up the ſulphur, and unites with it into ſcoria; which, by-the-bye, finely powdered, and duly waſhed, may be uſed with as good or a better effect, than the antimonium martiale cachecticum of Ludovicus. I have repeatly known it very ſerviceable in a leucophlegmatia, and an obſtinate fluor albus.

If crude antimony is firſt calcined by fire, as for making the glaſs, or boiled repeatedly in freſh ſtrong lime-water, much of the ſulphur is carried off, and the ſtibium, thus managed, yields much more regulus in proportion than if crude antimony had

had been flux'd; but the ſalts, uſed in ſuch caſe, ſhould be very conſiderably leſs in quantity than in the common method, and alſo previouſly reduced to the black flux. Monſieur Geoffroy ſays, the incinerated antimony, melted with about an equal quantity of black ſoap, gives down much more regulus than is to be had by the methods of Kunkel, Stahl, or any other, even nine or ten ounces out of a pound: but I really never tried this method, not having at hand any true black ſoap: perhaps ſome other ſalino-ſulphureous medium would do as well.

Upon the whole, however, this general obſervation may be made, that where only ſmall quantities of ingredients in this, or any other aſſay, are fluxed at a time, there will be always leſs regulus in proportion, than if the proceſs had been made with much larger quantities. Beſides, in truth, there is a great deal of difference in antimony itſelf, ſome abounding with reguline parts much more than others: nay, antimony, from the very ſame loaf, differs in this conſiderably, according as it is taken from the baſis or apex of the cone.

The effects of ſalts in antimony will more fully appear, if we attend to the operation of nitre in preparing the common liver of antimony; *viz.* if equal parts of antimony and nitre, finely powdered, and intimately mixed, are deflagrated, and melted in a crucible, or iron mortar, the product is a liver of antimony, which ſhould be ſeparated from the ſcoria. In this proceſs the ſulphur is firſt of all partly burnt off in the conflagration: and, 2dly, the remainder is eagerly imbibed by the nitre, now alcalized by the intenſe heat, which, by this means, alſo diſſolves,

and intimately combines with, the reguline part, juſt as common hepar ſulphuris diſſolves and takes up all kinds of metals. That this chiefly depends on the alcalization of the nitre, and its conſequent union with the antimonial ſulphur, and thus forming a diſſolving liver of ſulphur, that combines with the metallic part of the antimony, is evident; for three parts of ſalt of tartar, or pot-aſh, fluxed with two parts of antimony, produce exactly the ſame effect, that is a liver of antimony, without the leaſt admixture or help of any nitre. And it is from the intimate union of ſo large a portion of this hepar ſulphuris with the metallic part, that little or no regulus is depoſited, but only an uniform half-vitrified ſubſtance at the bottom: nay, if it be not ſufficiently fluxed, it gives off no ſcoria. However, if the pot-aſh and antimony are quickly melted with a very briſk ſtrong fire, a bit of regulus, ſometimes more, ſometimes leſs, is found at the bottom. But if a much leſs quantity of the alcalious ſalt is uſed, much leſs of the antimonial ſulphur is taken up, and it ſo forms what is called by Margraaf, Hoffman, and others, regulus medicinalis, that exerts but little emetic power, there being ſulphur enough left to invelope the reguline ſpicula. But when a much larger quantity of fixed alcali ſalts (as about two parts to one of antimony), is blended with it by fuſion, ſo much of the ſulphur is taken up by the ſalt, that little is left to ſheath the reguline parts ſufficiently to prevent their exerting a very draſtic power. The liver of antimony, made in the uſual way, with equal quantities of nitre, looſes almoſt twice as much in the operation, by the deflagration,

as

as that with the fixed alkali; but the latter is not ſo glaſſy, and much more apt to relent by the moiſture of the air. The crocus, however, from either, if perfectly edulcorated, is nearly of the ſame ſtrength. That indeed, prepared with half the quantity of nitre, is conſiderably weaker, as much leſs of the inveloping ſulphur is conſumed, eſpecially where a ſtrong fire is not uſed, and the matter is taken off before the ſcoria have well time to ſeparate. Even the ſcoria of the common regulus of antimony are of the very ſame nature, and a kind of an hepar antimonii, which by a proper flux may be eaſily reduced in part to a regulus: ſo that whether alkalized ſaltpetre, pot-aſh, or ſalt of tartar, are melted with this ſulphureous mineral, a liver of antimony is produced, from which, duly waſhed, a crocus metallorum, or, more properly, antimonii. And as theſe hepatic maſſes are ſoluble in boiling water, the ſolutions let fall an antimonial ſulphur, eſpecially when precipitated with a vegetable or mineral acid: but this ſulphur is always more or leſs impregnated with reguline particles, particularly that of the firſt precipitation, whence it is always conſiderably emetic. Nay, 'tis certain the reguline and ſulphureous parts of antimony may be ſo incorporated with fix'd alcali-ſalts, that the whole, almoſt, of any quantity of antimony, melted with about an equal part of ſalt of tartar, or pot-aſhes, may be thus turned into a kind of ſulphur auratum, as 'tis called; which is itſelf, in truth, no other than a very ſulphureous crocus antimonii. The coheſion however of the ſalt, ſulphur, and regulus, in this hepatic concrete, is eaſily diſſolved by pouring an acid to its ſolution in water,

which ſtrongly attracting the alcaline ſalt, the ſulphureous and reguline parts ſoon fall to the bottom. Nor are theſe latter very firmly united, as the alcalious ſalt did, during the flux, in ſome meaſure detach the antimonial ſulphur from the reguline parts. It is evident the coheſion is but looſe; otherwiſe ſo great a proportion of ſulphur, as is found in the ſulphur auratum, would, if very intimately combined with the reguline parts, have ſo ſheathed up theſe ſpicula, as to render them incapable of impreſſing any great irritation on the coats of the ſtomach, &c. as is ſeen in crude antimony, and its regeneration from ſulphur and regulus. Beſides, though the firſt precipitation of the ſulphur auratum is greatly impregnated with reguline parts, yet the ſecond or third precipitation holds exceedingly few, and is almoſt intirely ſulphureous, and ſcarce at all emetic. If you would have this milder, or more light ſulphur, you ſhould not pour on too much of the precipitating acid at firſt; or rather ſuffer the antimonial lixivium or ſolution to ſtand, for ſome days, expoſed in a cold open air; for thus the impure reguline ſulphur will fall of itſelf; after which you may inſtill the acid as uſual; and, if you do thus, by gentle degrees, in ſmall quantities, after the ſecond or third precipitation, the ſulphur will fall almoſt pure; which ſhews, that the ſulphur in the hepatic ſolution is not very cloſely united with the regulus. By the way, however, as the antimonial lixivium, from the hepar, is ſo fully fraught with reguline particles, and theſe ſo much unſheathed, it is conſtantly more or leſs emetic, notwithſtanding what Monſieur Lemery aſſerts to the contrary. But that is not the only exceptionable

thing

thing in that gentleman's elaborate treatiſe on antimony.

As the ſulphur auratum is now very frequently uſed in medicine, more exactneſs ſeems required in its preparation than is commonly practiſed: for certainly the firſt precipitation differs not a little from the ſubſequent, as being of a darker colour, and greater ſpecific gravity, and of courſe more reguline. 'Tis alſo of ſome conſequence how long the ſtibiate lixivium ſtands before the acid is poured on; for by time it will of itſelf drop much of the ſulphureo-reguline ſubſtance; eſpecially if it is kept in an atmoſphere much impregnated with the fumes of vinegar, ſulphur, or the like; and, in ſuch caſe, the ſucceeding precipitate, with an acid, will be much milder, as being leſs metallic, but more ſulphureous. The quantity and quality of the precipitating acid are alſo of conſequence. Vinegar may rather augment the emetic power; and ſpirit of ſalt, or vitriol, may make a precipitate not altogether ſo proper to mix with calomel, as in Dr. Plummer's alterative pill, *&c.* Great care ſhould be taken therefore in the ablution of the ſulphur auratum; for the acid ſalts are not ſo eaſily waſhed off as ſome would imagine, and it cannot be doubted but that ſome of them fall with the precipitate. Who would think ſuch a quantity of ſalts lay hid in the calx of ſilver, precipitated out of a ſolution of it in aqua fortis, by ſea ſalt, which though perfectly waſhed, and altogether inſipid (as is the luna cornea, into which it readily melts) yet two parts of this, intimately mixed with one part of regulus of antimony, and diſtilled, give a moſt cauſtic kind of but-

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ter of antimony. But however light and pure the antimonial ſulphur may be deſired, if it doth not in ſome degree participate of the regulus, it can have no more effect than common ſulphur.

The kermes mineral, once altogether as much celebrated (and with as good reaſon), as any antimonial noſtrum now-a-days, is alſo a kind of liver of antimony, and of the ſame nature with ſulphur auratum, though the proceſs in preparation ſeems very different. It is made by boiling crude antimony, powder'd, in a ſtrong lixivium of fixed or alkaliz'd nitre (ſalt of tartar or pot-aſh will do full as well). Here the alcalious ſalts fix on the ſulphur, and unite with it; whence a liquid kind of hepar ſulphuris, which diſſolves, or perhaps more properly takes up, and incorporates with, many of the reguline parts: and thus in reality it becomes a mild ſort of crocus antimonii, as is evident from its having ſome emetic power, and yielding a regulus by a proper flux.

There is another way of making a ſort of kermes mineral, or ſulphur auratum, ſeldom or ever practiſed, though I think really the beſt; and that is, by boiling crude antimony, finely powdered, in a very ſtrong lime-water, for about an hour, and then precipitating the ſtrained decoction with a ſolution of crude tartar, or its cryſtals, in boiling water. This decoction of crude antimony is as limpid as fountain-water, but gathers, by ſtanding, a thin film, moſt beautifully variegated with all the colours of rainbow. However, this perfectly limpid liquor, on the affuſion of a vegetable or mineral acid, immediately becomes turbid, and of a deep ſaffron-colour, and a great deal of an orange-coloured antimonial

ſulphur

ſulphur precipitates, juſt as in the common way of precipitating the decoction of the ſcoria of the regulus, or hepar antimonii. Though this ſulphur auratum, as well as the other, like the alchemiſtical gold, ſo much talked of, and expected, by the adepts, is much greater in proſpect than reality; for when duly waſhed, ſeparated, and dried, it ſcarce amounts to the fortieth part of what it ſeemed to be at the time of precipitation. I take this ſulphur to be rather milder, and more fixed, than that from the ſcoria: however, here alſo is an abundance of reguline parts, as is evident from the orange-colour, and the emetic quality, which this alſo poſſeſſes. This decoction may as well be precipitated with juice of lemons or Seville oranges (which give the brighteſt-coloured precipitate); or by a ſolution of ſal ammoniac, or freſh urine; even common ſaliva will do it; for, on taking a ſpoonful of this perfectly limpid decoction into my mouth, it in an inſtant turned intenſely yellow: ſo that not only vegetable and mineral acids will precipitate this antimonial decoction, but likewiſe neutral ſalts.

This decoction of antimony in lime-water ſerves alſo to a further end; for the mineral, thus boiled, may be in a great meaſure deprived of its ſuperficial ſulphur, eſpecially if boiled a ſecond or third time in freſh ſtrong lime-water. And then the powder, well waſhed and dried, is at fit for making regulus, or glaſs of antimony, as when the ſtibium is roaſted and calcined in the common way. And perhaps antimony, thus prepared, may be more effectual in many caſes, than the crude, when given internally:

it often pukes, and purges gently, if given to ten or fifteen grains, eſpecially on the firſt uſe of it.

It appears then, from what hath been ſaid above, that ſulphur auratum, kermes mineral, Ruſſel's powder, or Wilſon's panacea of antimony without fire, are all of the ſame nature, though indeed ſomewhat different in their ſtrength, and pretty uncertain in their operation; and therefore require a careful hand to prepare, and a good head to adminiſter them with advantage. For the reguline principle will be much more predominant in the ſcoriæ of the very ſame kind of regulus of antimony, if it is kept for a long time in a briſk fire, than it would be, if for a much ſhorter time; not to mention what I have ſaid before, as to the manner of precipitating, &c. the ſulphur auratum. And kermes mineral differs not a little, as prepared with a ſtronger or weaker lixivium, as more or leſs carefully waſhed and edulcorated, and as ſpirit of wine is, or is not, at laſt deflagrated with it. Whoever would give them, ſhould begin with ſmall doſes, as a grain or two; but, as he finds they agree, may gradually increaſe to eight or ten, eſpecially if they are intimately incorporated with any reſinous extract, natural balſam, or the like. But the doſes ſhould by no means too quickly ſucceed one the other; for ſolid antimonials may lie a conſiderable time in the body without any ſenſible effect, and yet, at length, operate all on a ſudden, with exceeding great violence; particularly when wine, cyder, or any vegetable acid, are ſwallowed upon them.

Though common ſalt, fluxed with antimony, ſeems to do little more than promote its fuſion,

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and therefore is commonly added, in ſmall quantities, in preparing liver of antimony, and regulus medicinalis, yet its highly concentrated acid hath a very peculiar effect on the reguline ſubſtance, rendering it not only much more volatile, but likewiſe exceſſively cauſtic, as is ſeen in common butter or oil of antimony; for, in this preparation, the ſublimate corroſive contributes nothing but its moſt highly dephlegmated and moſt penetrating acid ſalt, which the regulus more ſtrongly attracts than the mercury; and theſe, uniting, form a moſt corroſive liquid, which comes over by diſtillation; from which is precipitated, by the affuſion of common water, what is very improperly called mercurius vitæ, as it hath nothing of mercury in it but the name, and is, in truth, when duly edulcorated with boiling water, a mere regulus antimonii, as plainly appears when it is melted. Though there are ſeveral other ways (and theſe too leſs dangerous) of making butter of antimony; yet this proceſs with ſublimate corroſive ſhews the regulus naked as it were, when precipitated, and the antimonial ſulphur left behind with the mercury, which are eaſily ſublimed into cinnabar: ſo that this alſo confirms the above doctrine; for with well purified regulus antimonii and ſublimate corroſive no cinnabar can be prepared; there being no more ſulphur in the regulus than is barely neceſſary to preſerve the metallic form and conſtitution.

I have but ſeldom uſed mercurius vitæ in my practice, and that many years ago in ſome maniacal caſes: it always proved a very churliſh medicine, and I ſoon grew weary of it. If any one is inclined

to try any thing of this kind, I would recommend a preparation of the celebrated Dr. Stahl; *viz.* pour by little at a time, and ſlow degrees, near treble the quantity of good alcohol vini on rectified butter of antimony: as a conſiderable heat at firſt ariſes on the mixture, it immediately grows milky, and a very white gelatinous kind of maſs ſoon precipitates. This digeſted for a day or two, in a very gentle heat, then ſufficiently edulcorated with boiling water, dried and deflagrated with ſpirit of wine, gives a powder much ſofter in operation than the common mercurius vitæ, though ſtill emetic, and, as the Profeſſor ſays, greatly ſudorific and anodyne. The doſe three or four grains. I find it ſweats very largely, eſpecially when it operates little by vomit, or ſtool, as indeed moſt of the draſtic antimonials will do. Maets recommends another preparation of mercurius vitæ, under the title of purgans ex antimonio ſecuriſſimum; which is made by melting one part of mercurius vitæ with two parts of nitre, and then well grinding this maſs with an equal quantity of common ſalt. This done, let the ſalt be well waſhed off, and the maſs well edulcorated. This indeed I find ſo ſecure a medicine, that it differs very little in virtue from common bezoar mineral: the reaſon will eaſily appear to any one, who conſiders the following obſervations:

Antimonium diaphoreticum, and ceruſs of antimony, are little more than the dead aſhes of that mineral, deprived of its internal or metallizing ſulphur by repeated deflagrations with ſalt-petre; ſo that I think little can be expected from them in a medicinal way, both the one and the other being an inactive calx, in which the reguline form and con-

texture

texture are quite deſtroyed; inſomuch that aqua regia, which acts ſo readily on crude antimony, or its regulus, will not touch thoſe, much leſs diſſolve them: an argument, that the metallic nature of the antimony is greatly deſtroyed in theſe preparations. And though they are both reducible, by a proper flux, to regulus again, yet never without the addition of ſome phlogiſton, or ſulphureous ſubſtance. And thus indeed may any calx of antimony, prepared in the humid or dry way, be converted into regulus by fluxing in cloſe veſſels with ſome inflammable ingredient: which ſhews, that both the form and virtue of the regulus depend, in a great meaſure, on the ſulphureous principle, as well as the metallic earth. I know not whether it may be here worth noting by the way, that antimonial calx, reduced with mineral ſulphur, takes a ſtriated form; but with an animal or vegetable phlogiſton the luminous appearance of common regulus of antimony. Is this from the vitriolic acid only? Mercurius vitæ, fluxed *per ſe*, hath alſo this aculeated or needle-like appearance: whence?

laminate

This likewiſe is really the caſe in the moſt perfect metallic bodies, which looſe their metalleity, as Becher calls it, as malleability, and other metallic properties, by an intire deſtruction of their internal metallic or combining ſulphur, as is ſeen when metals are calcined by the burning-glaſs, or an intenſe culinary fire. This internal ſulphur is probably what Albertus Magnus means by the humidum unctuoſum ſubtile, which, he ſays, is the prima materia metallorum, and is intirely analogous to the combining ſulphur, or oil of vegetables, which binds the

very particles of the ſtamina, and organiſed parts together, and which, when totally burnt off, leaves the whole in aſhes. But here, by the way, it ſeems evident, that the ſulphureous pabulum, ſo neceſſary to the reduction of an antimonial calx, is very different from the very ſubſtance of light, or fire; for though a vaſt deal of the actual light, or fire, adheres to the calx, as is manifeſt from the great augmentation of weight in calcining regulus of antimony by the ſun-beams, or a culinary fire, yet it by no means contributes to its metallic ſtate; on the contrary it quite deſtroys it; and the ſun-beams, or particles of fire, do not combine, but ſcatter, the reguline or metallic parts, by deſtroying the bond of union, the internal ſulphur. In like manner oil of vitriol, or ſpirit of nitre, two concentrated and exceedingly fiery acids, tear abroad regulus of antimony, and reduce it to a calx, by letting looſe the phlogiſton, or ſulphureous principle, whence the metallic ſubſtance is quite decompoſed. And that this is the caſe, ſeems evident from the very ſtrong ſulphureous ſmell, that ariſes, when theſe acids are poured on antimony. Indeed oil of vitriol takes up the inflammable part of antimony, and unites with it into an actual mineral ſulphur. It ſeems then, that theſe fiery acids act on antimony very nearly in the ſame manner as the ſun-beams, or actual fire; and this is one argument, amongſt many others, of the very great affinity between light and acids; which Sir Iſaac Newton long ago hinted.

That the deſtruction of the internal ſulphur of the reguline ſubſtance, in theſe calcinations, is the deſtruction of the reguline form, and of courſe of the

emetic power of the antimony, appears in part from what I have ſaid above, and will be more manifeſt by attending to the following experiments. If merely equal quantities of antimony and nitre are deflagrated, and melted together, only ſo much of the external inveloping ſulphur is conſumed, as to leave the reguline ſpicula naked, and capable of very ſtrongly irritating the coats of the ſtomach, &c. But if three parts of nitre are taken to one of antimony, by the repeated ſtrong deflagration, not only the external but the internal ſulphur alſo is totally diſſipated, and the maſs reduced to an inert calx. Thus likewiſe iron, tin, and copper, with a double or treble quantity of nitre, are reduced to aſhes, and demetallized. A little more than two parts of pure nitre to one of regulus antimonii reduces it to an innoxious calx, as there is much leſs ſulphur to be burnt off in the regulus than in the crude antimony: and about an equal quantity of ſalt-petre quite deſtroys the virulence of glaſs of antimony, as in it there remained only juſt ſulphur enough to preſerve the reguline nature. So five parts of nitre to two of mercurius vitæ convert it into an active ſubſtance, or kind of bezoar mineral. But of this enough—What nitre doth by deflagration, the burning-glaſs doth by the intenſe force of the concentrated ſun-beams; by which antimony, its regulus, and glaſs, are turned to a mere calx, the whole of the ſulphur being quite burnt up. Nay, when any of theſe are a long time expoſed to the action even of a common ſtrong fire, they are reduced to mere aſhes, which can neither be run into regulus or glaſs again, but by the help of ſome phlogiſton, as animal, vegetable, or mineral ſulphur.

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And the ſame is neceſſary to the reduction of the calx of iron, tin, copper, &c. That there remains no ſulphur in any antimonial calx, ſo prepared, is manifeſt, in that no one of them will deflagrate with nitre, nor be reduced to a reguline ſtate, without adding ſome phlogiſton. That the reguline nature and contexture are deſtroyed in theſe preparations, appears in that they are not at all acted upon by aqua regia, which ſo perfectly and readily diſſolves any antimonial regulus: and hence alſo they are utterly devoid of an emetic quality.

But in all theſe calcinations with nitre, three parts at leaſt of that ſalt muſt be uſed to one of the crude antimony, or the internal ſulphur will not be ſufficiently burnt off, and the reguline ſpicula ſo far deſtroyed as to leave an inert calx: for if two parts only of ſalt-petre are employed, it proves ſtill emetic, as is ſeen in what Boerhaave calls antimonii emeticum mitius: ſo alſo if the nitre be conſiderably reduced, in the preparation of the ceruſs of antimony, that likewiſe will remain very draſtic. If equal quantities of nitre and glaſs of antimony are expoſed for ſome time, in a clean crucible, to a briſk fire, a very beautiful calx, or diaphoretic antimony, comes out, but altogether inactive: if two-thirds however, or half only, of nitre is uſed, the calx is nothing ſo white (the ſulphur not being quite burnt off); and thence it remains ſtill emetic, eſpecially if only half ſalt-petre is projected with the antimony.

Upon this foundation may be made ſeveral ſorts of antimonial powders, more or leſs active, as more or leſs nitre is uſed; which, prepared with care, and given with judgment, may prove of conſiderable

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ſervice in medicine, much more ſo ſurely than the common antimonium diaphoreticum, or ceruſs of antimony.

But, in all theſe calcinations, not only the quantity, but the quality alſo, of the ſalt-petre ſhould be well conſidered; for ſome nitre greatly abounds with common ſalt, and hence leſs corrects, as it is called, or rather deſtroys, the regulus.

But I greatly prefer the infuſion of the glaſs, regulus, or crocus of antimony, in ſound generous wine, to any other preparation of that mineral, as by far the moſt certain, ſafe, and effectual; and the vinum antimoniale made with the glaſs, or regulus, I think the beſt: for unleſs the liver of antimony is carefully prepared with a due quantity of nitre, and a proper degree of fire, it cannot be depended on, as being ſometimes ſtronger, ſometimes weaker, and ſometimes it throws up no ſeparable ſcoriæ; which makes it, *cæteris paribus*, conſiderably weaker. Beſides, the hepar ſhould be finely powdered, and well edulcorated with repeated ablutions (or rather decoctions) in hot water; otherwiſe much of the alcalized nitre will adhere to the hepatic crocus, and enervate the power of the vinous menſtruum. This was not formerly attended to ſo much as it ought to have been; and I well remember, when the vinum benedictum, as then called, was ſtrangely different the one from the other. Certainly, when prepared with true glaſs of antimony, or pure regulus, there is not this uncertainty. If the martial regulus is uſed, it may in ſome ſmall degree alſo participate of a ferrugineous principle. The wine ſhould be always carefully filtered, after ſufficient infuſion. In this

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infuſion of the glaſs or regulus of antimony on wine, the reguline ſubſtance is diſſolved by that ſaponaceous ſpirituous, tartareous menſtruum, and the reguline part becomes moſt highly attenuated; otherwiſe it would not ſo readily paſs with the wine through the cloſeſt filtre, and remain ſo long ſuſpended, and intimately incorporated with the wine, and ſo remain for years together. So that antimony, ſo prepared, is in ſolutis principiis, as it were; or rather is thus rendered a kind of highly ſubtilized liquid tartar emetic, which poſſeſſes the whole power and virtue of ſtibium, as it is potent enough to give a very ſtrong irritation to the ſtomach and inteſtines, if taken to the quantity of an ounce or two; and yet, in a ſmaller doſe of two or three drachms, it only cauſes a nauſea, gentle puke, and a ſtool or two, if neither a ſweat, or very high perſpiration: but, from thirty to ſixty or eighty drops, it generally proves merely an alterative and diaphoretic, paſſing through the inmoſt receſſes, and ultimate ramifications, of the whole vaſcular ſyſtem, with little or no diſturbance to nature, and yet evidently promotes all the animal ſecretions and excretions, particularly thoſe of the ſkin, inteſtines, urinary paſſages, and ſalival ducts, by gently irritating the whole nervous and vaſcular compages.

As this antimonial wine then ſo readily mixes with the blood and animal humours, and paſſes off ſo freely and eaſily through all the outlets of the body, it may be given with ſafety, and repeated with ſucceſs, two, three, or even four times in twenty-four hours, in ſmall doſes, and ſo continued for days together: whereas the ſolid antimonial preparations

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are very uncertain in their operation, ſometimes lying a long time in the ſtomach and bowels before they exert any ſenſible effect; and then, at once, irritating with ſo much violence and obſtinacy, that the patient is too often quite exhauſted before the force of the antimonial. And this is not a little to be feared, and ſometimes actually happens, when kermes mineral, ſulphur auratum, crocus antimonii mitior, and even regulus medicinalis, and other ſtibiate powders, or pills, are given, and too ſoon and frequently repeated, as every one muſt know, who hath freely dealt in ſuch ſort of medicines; a hypercatharſis ſometimes, all at once, and unexpectedly, coming on, eſpecially on drinking a glaſs of wine, cyder, or other vegetable acids.

Beſides, when antimonials are given in ſubſtance, they muſt firſt undergo a diſſolution in the ſtomach, before they can paſs the lacteals, and be mixed with the blood, ſo as to act as alteratives, diaphoretics, &c. Now, in this liquid preparation the reguline part is already diſſolved, and moſt exquiſitely attenuated, ſo that it paſſes into the blood with the utmoſt facility. It ſhould be moreover obſerved, that, in this form, antimony may be given in the moſt agreeable manner, without even being perceived, or creating any more diſtaſte than the wine it was made on. A thing of ſome conſequence truly, when we have to do with ſqueamiſh patients, particularly with children, to whom it may be neceſſary to give repeated doſes of the medicine. It is certainly then much in favour of this preparation, that it is ſo agreeable, and may be ſo eaſily concealed, and given in any proper liquor; and this, I ſay, to children, as well as grown perſons.

There is many times the ſtrongeſt indication for puking and purging them, and yet it is extremely difficult to give them the common medicines uſual on ſuch occaſions; whereas the antimonial wine may be given in their common drink with the utmoſt eaſe and ſafety. I have very frequently given it with ſucceſs, from ten to thirty drops, to children of a year or two old, in the chin-cough, and aſthmatic oppreſſions, when an attempt to force down more nauſeous medicines had endangered a ſuffocation and convulſions.

Should it be imagined, that this medicine, being ſo ſafe and eaſy, can have no great efficacy as an alterative and diaphoretic; I anſwer, that as it is capable, in a proper doſe, of irritating the ſtomach and inteſtines ſo ſtrongly, it cannot be ſuppoſed, even in very ſmall doſes, to lie inactive in the ſanguineous and lymphatic arteries; and both ſeems, and eventually is, exceedingly well adapted to ſtimulate and ſcour the whole vaſcular ſyſtem. In confirmation of this, we find, that when a pretty large doſe of the eſſence of antimony is given, as three or four drachms, for inſtance, a large ſweat almoſt always ſucceeds, if it is not immediately thrown up by vomit. By thus keeping up then, and quickening, the action of the veſſels on the contained fluids, the general circulation of the humours is moſt effectually promoted; and indeed ſome ſuch a ſtimulus is very often highly neceſſary in the ultimate ramifications of the ſanguineous, ſerous, and lymphatic arteries, where the motion is naturally exceeding ſlow, and where ſtagnation, and conſequent corruption of the ſerum and lymph, are very apt to generate a putrid colluvies.

colluvies. I think one of the uſes of ſalts of all kinds, eſpecially of common ſalt, is to act as a general ſtimulus; and hence the moderate uſe of it is ſo ſalutary. By thus univerſally ſtimulating therefore not only the greateſt, but alſo the ſmalleſt veſſels of the body, this medicine greatly tends to remove all obſtructions formed, or forming, even in the minuteſt canals; and hence moſt ſucceſsfully promotes the natural ſecretions and excretions; in which, duly performed, health itſelf conſiſts.

In obſtinate rheumatiſms then, in cold ſcorbutic affections, in moſt cutaneous diſeaſes, in aſthmatic, leucophlegmatic, and icteric diſorders, in old ſtubborn head-aches, vertigo, epilepſy, and mania, antimonials are very uſeful, and the vinum antimoniale in particular. In my own practice I have had numerous inſtances of its ſucceſs in the above caſes, and have likewiſe had the pleaſure of finding it ſucceſsfully uſed by ſeveral eminent practitioners.

Let me further add, before I quit the ſubject, that I very frequently give this antimonial wine, or eſſence of antimony, as I call it, in ſome acute as well as chronic diſorders, and particularly in ſlow fevers, low irregular intermittents and remittents, in catarrhal fevers, in a peripneumonia notha, and even in a true peripneumony, after proper evacuations, towards the cloſe, when the ſpitting is prematurely ſuppreſſed, and great anxiety and difficulty of breathing come on. In like circumſtances, it is very proper in the ſmall-pox alſo; and I have had the ſatisfaction, through divine goodneſs, of ſeeing it many times very happily ſucceed in many deſperate caſes; the expectoration returning ſometimes with a gentle

vomiting, ſometimes a ſtool or two, and ſometimes a univerſal kindly ſweat. The kermes mineral was formerly given in ſuch caſes with aſtoniſhing ſucceſs, and operated much in the ſame manner: however, I well know the eſſence of antimony is much more ſafe and certain; though the powder had a ſupriſing reputation in France and Germany, about thirty or forty years ago, and with juſtice too, when properly timed and doſed; but the too precipitate and injudicious uſe of it ſoon brought it into diſrepute. And this is generally the fate of all empirical medicines, which are cried up as good in all caſes, and at all times; for, though they may be really good in themſelves, under a proper adminiſtration, yet the indiſcriminate and imprudent uſe of them too often renders them poiſons inſtead of antidotes; as hath, in truth, been the caſe with ſome more modern arcana.

To conclude, I do not pretend that the obſervations I have here laid down are altogether new; I allow that far the greater part of them are commonly known, and as ſuch I produce them, without particular quotations from particular authors, or ſetting down proceſſes at large, which may be eaſily conſulted in the common books of chemiſtry; knowing alſo that there are ſeveral other ways of working, in the great way, much more commodious and cheap, by ſaving ſaltpetre, *&c.* But, if I miſtake not, I have, in ſome meaſure, given a new light into the nature of ſtibium; at leaſt have made it more obvious to the younger part of ſuch, whoſe buſineſs it is to prepare and exhibit antimonals, than they will readily find in any one ſingle treatiſe. And, as ſtibiate medicines are now ſo much in vogue, this little piece

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may not be an improper thing to be put into the hands of ſtudents in phyſic: perhaps it may excite even ſome of the more experienced to improve and aſcertain the virtues and doſes of antimonials, which at preſent are not a little undetermined.

Beſides, I had alſo a further view in drawing it up, which is to recommend the uſe of what I have called eſſence of antimony, or the vinum antimoniale (for the aromatic in it is of no great importance), as much the moſt ſafe and uſeful preparation thereof. I have uſed a great variety of antimonial medicines for near thirty years; and muſt ſay, from ſufficient experience, I greatly prefer this to any other, though I am far from condemning all the ſolid preparations of antimony; but I aſſert no one of them hath greater or better effects in medicine than this; and very few, if any, can be given with equal ſafety. I think I may ſay of many of them, in the words of Celſus, "His varie medici utuntur, "ut magis, quid quiſque perſuaſerit ſibi, appareat, "quam quid evidenter compererit."

But, after all, it is not this or that medicine, or preparation, will cure a diſeaſe, unleſs prudently made uſe of. A man may as perfectly well know how to make a hatchet, a hammer, or a ſaw, as a chemiſt how to make ſuch or ſuch particular medicines; and yet the firſt may be as far from being a good carpenter, as the ſecond from being a good phyſician: the arcanum is how to uſe them.

CVI.

CVI. *An Account of Mr. Samuel Tull's Method of caſtrating Fiſh. Communicated by* W. Watſon, *F. R. S.*

To the Royal Society.

Gentlemen,

Read Dec. 19. 1754. SEveral years ſince, Mr. Tull of Edmonton performed the operation of caſtrating fiſhes, before Sir Hans Sloane, Bart. and ſeveral members of the Royal Society, who met at Sir Hans's houſe for that purpoſe. About five or ſix years ago he performed the ſame operation in preſence of our late worthy Preſident Mr. Folkes, myſelf, and ſeveral others. Among theſe was a very valuable member of this Society, Mr. Trembley, who is with the Duke of Richmond on his travels, and is now at Feltzberg in Auſtria, at a country-ſeat of the Prince of Lichtenſtein. This nobleman, who is a great lover of natural hiſtory, as well as a zealous promoter of uſeful knowlege, is very curious in whatever relates to the hiſtory of fiſhes; and being informed by Mr. Trembley, that he had ſeen fiſhes caſtrated in England, and of the advantages which he had been aſſured did reſult from it, his highneſs intreated him to procure him from hence the moſt exact account of the manner of performing this operation, and of ſeveral particulars relating thereto.

Mr. Trembley accordingly wrote me a letter, deſiring me, if poſſible, to tranſmit to him this information. In conſequence of which, I wrote to Mr.

Tull, who in a few days was ſo obliging as to anſwer my letter, and to ſend me what I had requeſted of him, and which I accordingly have tranſmitted to Mr. Trembley, for the Prince of Lichtenſtein. But as nothing of this ſort has been communicated to the Royal Society, as a body, I thought it not improper to lay before you Mr. Tull's account of his operation; that, if it be thought worthy, it might be recorded in the journals and regiſter-books of the Society.

In England, where in many parts ſea-fiſh are in great plenty, the fiſh of rivers and ponds are leſs eſteemed; and improvements, either with regard to their bulk or increaſe, are leſs attended to: but in Germany, remote from the ſea, where pond-fiſh are a great article of traffic, Mr. Tull's method may be of great uſe.

Mr. Tull informs me, that he caſtrates both the male and female fiſh; and that, although almoſt any time is proper for the operation, the leaſt ſo is juſt after they have ſpawned, as the fiſh then are too weak and languid to bear, with ſucceſs, ſo ſevere an operation. The moſt eligible time however is when the ovaries of the female have their *ova* in them, and when the veſſels of the male, analogous to theſe, have their ſeminal matter in them; inaſmuch as at this time theſe veſſels are more eaſily diſtinguiſhed from the ureters, which convey the urine from the kidneys into the bladder, and are ſituate near the ſeminal veſſels on each ſide of the ſpine. Theſe may, without ſufficient attention, be taken for the ovaries; and the more ſo, when theſe laſt are empty. When fiſhes have ſpawned a few weeks,

weeks, they are fit for the operation; for, like hens, they have ſmall eggs in their ovaries as ſoon as they have laid their former clutch of eggs.

When a fiſh is intended to be caſtrated, it muſt be held in a wet cloth, with its belly upwards; then with a ſharp pen-knife, with its point bent backwards, or other well-adapted inſtrument, the operator cuts through the integuments of the rim of the belly, and in doing this he carefully avoids wounding any of the inteſtines. As ſoon as a ſmall aperture is made, he carefully inſerts a hooked pen-knife, and with this he dilates this aperture from between the two fore-fins, almoſt to the anus. From the back of this inſtrument, being blunt, the danger of wounding the inteſtines is avoided. He then, with two ſmall blunt ſilver hooks, of five or ſix inches long, and of the * form hereunder deſcribed, by the help of an aſſiſtant, holds open the belly of the fiſh; and, with a ſpoon or ſpatula, removes carefully the inteſtines from one ſide. When theſe are removed, you ſee the ureter, a ſmall veſſel, nearly in the direction of the ſpine; and at the ſame time the ovary, a larger veſſel, lying before it, that is, nearer the integuments of the belly. This laſt veſſel you take up with a hook of the ſame kind with thoſe before-mentioned, and detaching it from the ſide far enough for the purpoſe, divide † it tranſverſely with a pair of ſharp ſciſſars; remem-

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† Mr. Tull has frequently, to prevent the re-union of the divided ovaries, by which the effect of the operation might be defeated, taken out part of them, and nevertheleſs the fiſh have ſurvived.

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bering always, that great care is taken in not wounding, or otherwiſe injuring, the inteſtines.

After one of the ovaries has been divided, proceed in the like manner to divide the other; and then ſew up the divided integuments of the belly with ſilk, inſerting the ſtitches at a very ſmall diſtance one from the other.

Mr. Tull firſt put this method into practice, in order to prevent the exceſſive increaſe of fiſh in ſome of his ponds, where the numbers did not permit any of them to grow to an advantageous ſize. But from caſtration the increaſe was not only prevented, but the caſtrate fiſh, as Mr. Tull aſſerts, grew much larger than their uſual ſize, were more fat, and, which is no trifling conſideration, were always in ſeaſon.

He obſerves further, that the ſpawning-time is very various: that trouts, for inſtance, are full about Chriſtmas; perch in February; pikes in March; and carp and tench in May. You muſt always, however, make ſome allowance for climate and ſituation, with regard to the ſpawning of fiſh. And, from a very diligent attention, he aſſures me, that he has been able to ſettle a point much controverted by naturaliſts, in relation to the copulation of fiſhes. The moſt generally received opinion has been, that they did not copulate; but that the female did caſt her ſpawn in the water, and that then it was fecundated by the ſpermatic matter of the male. Mr. Tull, in contradiction to this hypotheſis, aſſerts, that he has frequently ſeen fiſhes in actual copulation; and that this is generally done before the ova arrive at maturity.

After Mr. Tull has caſtrated his fiſh, they are put into the water where they are intended to continue. He makes no particular appropriation, neither with regard to the ponds into which they are put, nor does he give them any particular nouriſhment; but they take their chance in common with other fiſh, as though they were not caſtrated. And he informs me further, that if tolerable care is taken, very few fiſh die of the operation, when performed in the manner here deſcribed; though heretofore, when, inſtead of the belly, he made the opening in the ſides of the fiſh, numbers died, from his wounding the inteſtines, and frequently dividing the ureters.

I am, with all poſſible reſpect,

Gentlemen,

London, Dec. 31, 1754.

Your moſt obedient humble ſervant,

William Watſon.

The following Addreſs having been read to this Royal Society, the Preſident and Council of the ſame, being deſirous to encourage, as much as may be, an Undertaking ſo uſeful, deſire the ſeveral Members thereof, and their learned and ingenious Correſpondents, to communicate their Obſervations, directed to their Secretaries, in Crane-Court, Fleet-Street.

CVII. *An Attempt to point out, in a conciſe manner, the Advantages which will accrue from a periodic Review of the Variation of the magnetic Needle, throughout the known World; addreſſed to this Royal Society by* William Mountaine *and* James Dodſon, *Fellows of the ſaid Society, and requeſting their Contribution thereto, by communicating ſuch Obſervations concerning it, as they have lately made, or can procure from their Correſpondents in foreign Parts.*

London, March 20, 1755.

Read March 20, 1755.

ABOUT the year 1700, the juſtly celebrated Doctor Edmund Halley having collected together a great number of obſervations, made on the variation of the needle, in many parts of the world, drew (on a mercator CHART) certain lines, ſhewing the quantity of that variation, in thoſe parts of the world, over the repreſentation of which thoſe lines were drawn; but as the quantity

of this variation is in a perpetual ſtate of fluctuation, in (perhaps) every part of the world, it had been ſo much changed in the ſpace of forty years, or thereabouts, that (when the writers of this paper endeavoured about the year 1744, to draw thereon, other lines to anſwer the purpoſes above-mentioned) they found that thoſe laid down by Doctor Halley were grown intirely uſeleſs; and that a ſyſtem of ſuch lines, or ſomething analogous thereto, ſhould be performed once in every ten or twelve years at leaſt, in order to anſwer the purpoſes intended, by that ſagacious gentleman.

In the reconſtruction of them, the writers received the aſſiſtance of the commiſſioners of the navy, and of the directors of the Eaſt-India and African companies, having leave to peruſe the journals of thoſe mariners, which were under the direction of each reſpective body; From theſe, and a few private communications, they were enabled to draw the proper lines over the moſt frequented ſeas, and to make ſome attempts toward doing the ſame in thoſe leaſt ſo; A copy of the CHART, thus again rendered uſeful, they preſented to this Royal Society with an account of the methods uſed in performing the ſame.

Although the moſt beneficial uſe of theſe lines belongs to the ſea, yet if they could be extended over the land likewiſe, the advantages ariſing would more than compenſate the trouble, as will appear by taking a ſhort view of each.

And firſt, the uſe of theſe lines at ſea may be conſidered either as common to the art of navigating in all large bodies of water; or as particular in ſome ſuch: the general uſe being that of ſteering the true

courſe

courſe deſigned, and finding the ſhip's true place, as near as may be, by what the mariners call the dead-reckoning.

The particular uſes will be beſt explained by examples; for inſtance, in the ſouthern parts of the great Atlantic ocean, beginning with the coaſt of Brazil and Patagonia, and proceeding to the ſouth of the Cape of Good Hope into the Indian ocean, as far as the common tracks of our Eaſt-India ſhips extend, the variation lines have appeared to be, for the moſt part, directed northward and ſouthward; whence, in moſt places of that great body of waters, if the latitude and variation be found by celeſtial obſervations, the longitude will be obtained by the lines on the chart; the great uſefulneſs of which has been atteſted to the writers, by many perſons who have, ſucceſsfully to themſelves, practically applied the laſt conſtructed CHART, to correct their dead-reckoning on that long paſſage.

Indeed, where the variation lines run nearly eaſtward and weſtward, as has appeared in the Atlantic ocean, from the weſt coaſt of Europe to the eaſt coaſt of North America, no aſſiſtance toward obtaining the longitude can be derived from them; but as it frequently happens, within thoſe limits, that meridian obſervations, for determining the latitude, cannot be obtained, eſpecially about Newfoundland; then, if a good obſervation of the variation can be taken, at any time of the day, the latitude may be nearly aſcertained by the lines on the CHART.

Secondly, The advantage that will ariſe by extending the variation lines over the land, as well as ſea, will be the confirmation of thoſe drawn over the

waters;

waters; the continuation of which, from ſea to ſea, will be thereby conſpicuous, and we ſhall be enabled to judge better of their nature, properties and cauſes; and, if the ſame can be extended over all the parts of the known world, the eye will be preſented, at one view, with the different degrees of attraction, with which all the parts of this great magnet are endued, at the time when ſuch lines are drawn; This the writers would have attempted to have done, in the year 1744, if they could have procured a ſufficient number of obſervations for that purpoſe; but although they frequently advertiſed their requeſt, in the public papers, no aſſiſtance was thereby obtained.

As the writers have by experience found, that the proper period for re-examining the ſtate of the variation is now at hand, without which the above-mentioned valuable advantages of the CHART will be loſt to the mariner; they have determined to collect and compare all the obſervations that can be procured by them, in the ſpace of a year from this time, or ſo long after as the return of the Eaſt-India ſhips then next following; if ſuch delay ſhould become neceſſary, by the ariſing of any doubt in conſequence of ſuch compariſon; and then to publiſh the reſult of their proceſs, in ſuch a manner as ſhall ſeem moſt convenient.

Several of the learned and ingenious have endeavoured to account for this phænomenon of the variation of the magnetic-needle, and the continual mutation thereof; whence different methods of computation have been propoſed, whereby they have endeavoured to determine what the quantity of the

variation

variation will (according to their ſeveral hypotheſes) be at any given place and time: The above propoſition, therefore, will (if carried into execution) bring theſe ſeverally to the teſt, and enable the judicious either to approve or reject them; the writers being determined (as was their former plan) to publiſh nothing which ſhall not be warranted by the real obſervations, which ſhall come into their hands, and ſhall leave the application thereof (as to each hypotheſis) to others: if any of them ſhould be ſo far confirmed, by this examination and compariſon, as to give juſt ground for a calculation, their labour will be at an end; but if not, they humbly recommend the continuance of ſuch a periodic operation, as they now propoſe to undertake, being the only means of attaining ſuch a deſirable event, and of ſupplying the defect till it can be obtained.

To whom then can they ſo properly apply, as to the Royal Society of London, for aſſiſtance, in a work of ſo much conſequence to trade and navigation, and from whence ſo valuable an addition to natural knowledge may poſſibly accrue: It has been the peculiar honour of the many illuſtrious members of this body, that they have deduced their knowlege from experiments, and not from hypotheſes; and (as that is the method now propoſed) the writers of this paper humbly deſire of this Society, as a body, and of each individual that compoſes it, to communicate to them ſuch obſervations of the variation as they have already made within a few years laſt paſt, or ſhall hereafter make, before the 25th of March 1756, either by ſea or land; and (as all mankind

are

are equally interested in this research) that they will endeavour to procure the like favour from their several correspondents in foreign countries: They beg leave to assure the Society, and its particular members, that they shall receive a grateful acknowledgement of this favour, granted; and an early information of any thing relating hereto, which the writers shall conceive to be worthy their attention.

***The END of* VOL. XLVIII.**

Additions

Additions *and* Corrections *in* Vol. XLVIII.

PAGE 19, *to the end of the third paragraph relating to Sir Iſaac Newton's Chronology, add this Note:*

I think myſelf bound, for the ſake of truth, to ſay, that I was miſinformed in what I have ſaid here concerning Sir Iſaac Newton's Chronology; for I find ſince, that the five firſt chapters of that work had received his finiſhing hand; that he had reſolved to publiſh it in his life-time; and that it was only the *ſhort Chronicle*, which had been ſurreptitiouſly tranſlated and printed in France. I am greatly obliged to the preſent Lord Biſhop of Bangor for this intimation, ſupported by many curious particulars of what paſſed in converſation on that head between Sir Iſaac and his Lordſhip.

Page 534. *l.* 11. *dele the ſemicolon after* vane, *and inſert it after* broad. —*p.* 535. *l.* 23. *inſert a comma after* agate, *and dele that after* likewiſe.—*p.* 536. *l.* 16. *for* fourth *read* fourteenth.—*p.* 602. *l.* 9. *after* contact *add a colon*; *l.* 11. *inſtead of a ſemicolon after* wood, *inſert a comma, and the ſemicolon after* varniſh'd. —*p.* 604. *l.* 29. *for* the other end acts *read* the other end of the laſt-mentioned bar acts.—605. *l.* 19. *for* plate *read* place; *l.* 27. *for* $\frac{1}{2345}$ *read* $\frac{1}{23145}$.—*p.* 609. *l.* 3. *for* I *read* T; *l.* 19. *for* I *read* T.—*p.* 610. *l.* 1. *dele* A Table of; *l.* 2. *for* Col. 6. *read* Col. 7.; *after* determined *add* as alſo that in No. 3 Col. 5.

In TAB. I. *between p.* 610 *and* 611. *in the Title of the Columns* 9 *and* 12. *for* different parts *read* difference of parts.

In the ſame Table, Col. 1ſt, 2d, 3d, 4th, 5th, 6th, 7th, and 8th, are ſufficiently explained by the titles upon them in the Table. Col. 9th is the difference of the numbers in columns 3d and 7th, and expreſſes the differences of expanſion of the baſis and the bar meaſured, between the different degrees of heat expreſſed in the 10th column; which column conſiſts of the differences between col. 4th and 8th.—— Col. 11th is made by proportioning the numbers in col. 10th, to what they ought to be, upon the ſuppoſition of an uniform expanſion, in caſe the difference of heat had been conſtantly 166 degrees of Farenheit.— Col. 12th, 13th, and 14th, are made in the ſame manner, from columns 3d and 5th, 4th and 6th, as columns 9th, 10th, 11th, are made from columns 3d and 7th, 4th and 8th.—— Col. 15th is made by ſaying as 166° is to 66°, ſo is any number in col. 11th to its correſpondent one in col. 15th; which, was the expanſion quite uniform, or in the ſame ratio as that of quickſilver, ought to agree with col. 14th.—— Col. 16th contains the differences of col. 14th and 15th, which are the irregularity of the expanſion,

panſion, or the deviation of the numbers in col. 14th, from what they ſhould have been, in caſe the metals had expanded equably.——*N. B.* If the numbers in col. 11th are added or ſubtracted, according to the ſigns in col. 9th, to or from the conſtant number 287.5, which is the expanſion of the baſis, this will give the real expanſion of the bars in parts of the micrometer, by a difference of heat equal to 166 degrees of Farenheit; taken ſomewhat above freezing, and below the boiling heat.——The expanſion of the baſis for 66° above the lower heat before-mentioned is 114.5. This alſo is a conſtant number; from or to which if the numbers in columns 14th and 15th are added or ſubtracted, according to the ſigns in column 9th; the real expanſions will be given for 66°; the firſt as it turns out from experiment, the other as it ſhould turn out upon a ſuppoſition of equable expanſion; but as the numbers ſo obtained from columns 14th and 15th will have the ſame arithmetical differences as the numbers in 14th and 15th columns have, the irregularity, in proportion to the whole expanſion, will be very little; and in general ſo ſmall that it may in a great meaſure be owing to ſome inaccuracy in making the experiments themſelves.

Page 696. line 25. *for* ליקרא, *read* ליקרה, or ליקרא,— p. 695. l. 4. p. 707. l. 7, 29. p. 715. l. 10. *for* ליקרא *read* ליקרה— p.708. l. 4. *for* SENATVS POPVLVSQVE *read* SENATUS POPVLVSQVE HONORIS CAVSA— p. 708. l. 11. *for* DECREED HIM, *read* DECREED HIM, IN ORDER TO DO HIM HONOVR,— p. 717. l. 7. of the Hebrew inſcription *for* יסהבית *read* אסהבית— p. 724. l. 23. *for* eighth *read* ſeventh—— p. 733. l. 25. *for* ſame, *read* ſame p. 735. l. 6. *for* Romans. *read* the Romans.— p.735. l. 19. *for* writers; *read* writers,— p. 736. l. 4. *for* MALACHBELVS *read* MALACHBELVS;— p. 737. l. 20. *for* of which *read* of which,— p. 739. l. 1. *for* admit, *read* admit— p. 748. l. 5. *for* Nu *read* Ny— p. 750. l. 18. *for* Aglibolus, and Malachbelus, *read* Aglibolus and Malachbelus,— p. 750. l. 25. *for* Baal Shemeſh. *read* named him Baal Shemeſh.

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